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EDITORIAL

A PROPOSED SYMPOSIUM ON A PROGRAM FOR PARAPSYCHOLOGY

THE OUTLOOK today is a hopeful one for parapsychology. Interest in this newcomer among the sciences is more widespread than it has ever been before. A secure foothold has been obtained in university laboratories, and collegiate experimental work is spreading. Research workers in the psychical research societies have been making substantial contributions. Professional people in neighboring fields—psychology, psychiatry, anthropology, and even in physics—have in increasing numbers manifested active interest in the new developments from the experimental laboratory in parapsychology.

The time, then, is one for taking stock. For the students and workers in the field who have thorough acquaintance with developments, certain important findings have been established beyond all reasonable doubt. For example, none of us is interested further in merely adding more evidence of the clairvoyant type of ESP. Moreover, those of us who have spent long years in the accumulation of great amounts of PK evidence are disinclined simply to go on repeating that task. Even with respect to the more challenging phenomenon of precognition it is necessary to raise new issues to excite anew our urge toward further experimentation. On the other hand, since the question of the requirements for evidence of telepathy has

been reopened, confirmation is needed for the McMahan experiment at Duke. Likewise, the pioneering beginnings made by Humphrey, Schmeidler, and Stuart on the personality correlates of psi capacities need to be carried forward to the discovery of more definite underlying principles. And we need also to follow through the problem of how psi capacities can appear to be independent of space-time-mass relations and yet somehow interact with physical target objects in both ESP and PK.

What directions are most appropriate for the researches of the immediate future? What is the most nearly ideal program for the explorers of this field to set up for themselves for the period, let us say, of the next ten years? How freely are we now entitled to range in our inquiries into still other claims beyond those with which we have dealt experimentally in the past? What new problems may we now dare to tackle without endangering the degree of confidence in our scientific judgment which has been so painfully gained through years of patient and cautious effort? And what further advances are we now equipped to undertake?

There will not be general agreement about the answers to these questions. Yet it may be profitable to try to find as much agreement as we can. Perspective is, of course, enormously important in launching research activities: we do not plan beyond our range of vision.

It is especially important that in the coming years we do not allow ourselves merely to drift on our course of research. A glance backward over the history of parapsychology reveals too little of orderly development and too much unplanned following-up of striking claims and of facile procedures. We may well excuse this absence of a program in the science's early years, for it is probably typical, but we must not unnecessarily prolong the confusion. It is time we became aware of where we are heading and of how well we are investing our research efforts.

The editors of the JOURNAL believe, therefore, that it would be a service to students and experimenters in parapsychology to have a wide range of judgments as to what we need to do now in our field. We have decided as a first measure to ask a number of the more

mature workers and scholars in parapsychology here and abroad who have been active during the last ten years to present for publication in the JOURNAL what they envision as the best program for this branch of research for the next decade. What we shall ask for is a concrete, realistic judgment of what we need most to investigate within the range of those phenomena which the parapsychological inquirer can attack with reliable methods of research. If we overlook, as we doubtless shall, any who have an active interest in this program, our correspondence section is always open to them if they wish to make use of it.

We propose to publish these statements as they become available, beginning with the next number of the JOURNAL.

J. B. R.

A SECOND ZAGREB-DURHAM ESP EXPERIMENT

By Elizabeth A. McMahan and J. B. Rhine

ABSTRACT: After an interval of almost seven years, another experiment has been carried out between Zagreb, Jugoslavia, and Durham, North Carolina. The subject was Dr. Karlo Marchesi, who was located in Zagreb. Every day, as in the previous experiment, he called 10 decks of ESP cards which were set up in the Parapsychology Laboratory in Durham. In calling 354 runs, he obtained 89 hits more than the chance expectation, a result which indicates that ESP was operating. The principal value of this experiment lies in the fact that significant results were again obtained over a 4000-mile distance, the greatest over which ESP has been tested. It gives added evidence that ESP is not limited by space.—Ed.

In 1939 and 1940 an ESP experiment was carried out between Dr. Karlo Marchesi, a physician located in Zagreb, Jugoslavia, and the Parapsychology Laboratory at Duke University in Durham, North Carolina. The test followed the card-calling method known as DT, in which the cards are left untouched throughout the test. In one series, reported as Section I, Dr. Marchesi attempted to identify cards set up in the Parapsychology Laboratory. In a second series, Section II, 10 American subjects attempted to identify similar decks of cards set up by Dr. Marchesi in Zagreb. The experiment, reported by Rhine and Humphrey in this Journal in 1942 (2), gave results ascribable to chance in so far as the total number of hits was concerned. Both sections taken separately gave total scores above expectation, but not significantly so. Only in the last of his four subseries, one that was interrupted by the outbreak of the war did Dr. Marchesi obtain a score significantly above expectation. That series showed a positive deviation of 34 hits on 35 runs through the deck, which gives a critical ratio of 2.87.

But while the total deviation of Dr. Marchesi's work as a whole was insignificant, the distribution of hits on his record sheets, when they were analyzed in the Parapsychology Laboratory, showed such lawful position effects as to make his part of the work highly sig-

nificant. It was found that the salience of the scoring for the five segments of the 25-trial run correlated to a significant degree with the corresponding salience of the five trials within the segment itself. The probability that this covariation occurred by chance alone is .00014. Even after all appropriate corrections were made, it still gave the experiment an extrachance classification. Dr. Marchesi had demonstrated ESP over a transoceanic distance.

When, at the close of World War II, we were able to resume correspondence with Dr. Marchesi, he indicated his desire to continue the interrupted experiments. The research we are about to describe was the result. Dr. Marchesi suggested the introduction of precognition into a part of the experiment and also indicated his interest in recording the meteorological conditions (at Zagreb) accompanying the test. These changes were adopted, and a few minor modifications were introduced by the writers, as will be seen in the account of the procedure which follows. The principal changes consisted of adding a drawings test to the card work and of introducing five colored cards into each deck.[1]

The Experiment

At Dr. Marchesi's suggestion, three series of tests were planned: two in which the cards were to be called on the day on which they were set up, and a third in which they were to be called a day in advance. These three series were carried out between Novmber 18, 1946, and January 16, 1947, with a pause of a few days between series. Ten decks of the ESP cards were called daily except for certain specified holidays. The first two series, A and B, extended through 15 test days, and the last series, C, through 20.

The regular ESP cards were used in the experiment, and each deck was enclosed in a small cardboard box, with the five colored cards—one of each symbol—substituted for five of the regular black and white cards in each of the 10 decks. These colored cards were taken from the No. 2 type of deck having red crosses, blue waves, yellow circles, green stars, and black squares. Since the black squares were not different from the squares in the regular deck, they were

[1] Because of difficulties of correspondence at the present time, Dr. Marchesi has not had an opportunity to see the manuscript before publication and should not be held responsible for any of its conclusions.

made to appear three-dimensional by the addition of appropriate lines in black ink. Dr. Marchesi was not told about the colored cards; he was merely informed that we were initiating a certain slight change in the procedure followed in 1939, a change that would make no essential difference in the procedure he was to follow but which would make the interpretation of the results a little more interesting.

In planning the experiment, we proposed the already mentioned drawings test as an addition to the card runs, and Dr. Marchesi agreed to make a drawing each day in response to a stimulus picture to be set up along with the cards. After the experiment was finished, however, Dr. Marchesi was unable to get the drawings through the mail to us and we are therefore unable to present the results of this part of the experiment. The data given in this paper (354 card runs) are all the work that Dr. Marchesi has been able to do since renewing contact with us. After completing Series C, he was unable because of his situation in Jugoslavia to continue the tests.

Although the technique used in Series A and C is for convenience called DT, it differed somewhat from the usual DT procedure, in which the cards are called before being looked at. The order of the cards was recorded by E. McM. as the decks were set up, but no attention was given to them or to the record thereafter. We think, therefore, that in effect and in the mind of the subject, the test was closer to the DT than to the GESP procedure. GESP, strictly speaking, is the term applied to tests in which the card or other stimulus object is looked at by the experimenter while being called by the subject.

The targets were set up in the same manner for both the DT and the PDT Series. The only difference in the two tests was the fact that in the latter Dr. Marchesi made his responses one day in advance of the actual target set-up. For example, on December 4, he called the order of the cards for December 5 and drew his version of the picture that was to be exposed on that day. (The PDT results are open to interpretation in terms of ESP on the part of E. McM. That is, by ESP she could have known the card order already predicted by Dr. Marchesi, and could unconsciously have shuffled the target deck to match it.)

The room in which the targets were set up was J. B. R.'s office. A small table stood against the west wall, and it was here that every day at 10:00 P.M., Greenwich Mean Time (5:00 P.M., E.S.T.) each deck was shuffled and recorded on a regular record sheet by E. McM. The colored cards were not spaced regularly but were shuffled indiscriminately with the rest. E. McM. recorded the decks on a printed record sheet, circling the colored symbols in order to distinguish them from the others. Each deck was placed face-down in its cardboard box, the lid was closed, and the box was propped against the wall so that the cards faced west. The boxes were numbered on the lids from 1 to 10 and were placed in this order from left to right. The record sheet was tacked to the wall directly over the cards.

From the Laboratory's collection of stimulus picture material E. McM. next selected at random the picture to be used as target for the day and fastened it above the record sheet.

E. McM. was in charge of setting up the targets except for the period from December 26 to January 2 when she was away. J. B. R. took over the task during that time.

It was decided before the experiment was begun that the targets would be changed every day except Sunday. For this day the targets for Saturday would continue to serve.

Results

After all the record sheets had been received from Dr. Marchesi they were compared with the target sheets, and the hits were checked.[2] The first analysis was for total results and these may be seen in Table 1.

The uneven number of runs is due to the fact that in a few sessions Dr. Marchesi was interrupted before completing 10 runs, and on several days he was unable to do any runs at all. As Table 1 shows, the PDT runs have a slightly higher average run score (5.33) than have the DT runs. (When the two DT series are pooled,

[2] We made duplicate copies of the record sheets containing the card order, and Dr. Marchesi did the same for his call sheets. Because of difficulties of mailing, and in order to be certain that we received the call sheets, Dr. Marchesi sent the two copies separately. All those sent for Series A and C arrived safely; for Series B, all the duplicate sheets arrived, but only seven of the original sheets.

Table 1

TOTAL RESULTS

Series	Runs	Dev	Av. Score	CR
Series A (DT).....	103	+13	5.13	.64
Series B (PDT).....	110	+36	5.33	1.72
Series C (DT).....	141	+40	5.28	1.68
Total.....	354	+89	5.25	2.37 P = .01

the average run score is 5.22.) This difference, however, is not significant.

The CR of 2.37 for the three series combined is just significant, with a probability of 1 in 100.

When the colored cards are considered alone, we find there are 70.8 runs, with a deviation of +22. The average score for these special card trials is slightly higher than it is for the rest of the cards (5.31 as against 5.24), but again the difference is not significant.

The data were next analyzed for position effects; that is, the hits were studied in relation to their position on the page. The horizontal distribution for the three series pooled is given below. It shows the runs and deviation for each of the 10 run positions across the record page. There do not appear to be any well-defined or significant trends.

Order of Run:	1	2	3	4	5	6	7	8	9	10
No. of Runs:	37	37	37	36	35	35	35	34	34	34
Total Dev.:	−7	+14	+32	−11	+16	−1	+12	+24	+10	0
		+7		+21		+15		+36		+10

The vertical distribution, or the number of hits for each trial in the record column, was obtained for the work as a whole. This run distribution was then reduced to five subtotals, one for every five-trial segment of the run, and another distribution was made for the five trials within the segment. Thus we obtained a five-point run and a five-point segment distribution based on the same 25-trial column.

This vertical distribution requires a word of explanation. Dr.

Marchesi, for the two DT series, guessed the order of the cards in a reverse manner, beginning his recording at the bottom of the record sheet and going up. This procedure was apparently caused by a slight misunderstanding of the instructions. The PDT targets, however, were guessed in the regular fashion. Keeping this in mind, we arranged the deviations in the vertical distributions for the two DT series so that the last trials in the column (representing Dr. Marchesi's first trials) are at the top in the distributions given below. The PDT runs are totaled with the DT runs. Each of the five deviations represents 70.8 runs. There are vertical declines in both the run and the segmental distributions, but neither is significant.

Segments in Run		Trials in Segment	
+17		+23	
+24	+41	+32	+55
+18		+17	
+13		+ 6	
+17	+30	+11	+17

The salience ratios for the three series were computed, both for the segments in the run and for the trials in the segment, and the covariation between the two ratios was evaluated. (The salience ratio is the sum of the squares of the CR's of the two end positions divided by the sum of the squares of the CR's of the three middle positions.) The salience ratios for both the segment and the run showed middle salience, as had been the case in the earlier Marchesi work; that is, the rate of scoring in the three middle trials of the segment was higher than that of the two end trials, and also the rate of scoring in the three middle segments of the run averaged higher than that of the two end segments. The covariation, however, was in the present case insignificant, though it had been very significant in the earlier research.

On the backs of the record sheets Dr. Marchesi noted the barometer, temperature, and hygrometer readings in Zagreb for each day.[3] Table 2 shows the results of the analysis made to see what effect, if any, weather conditions had upon the scoring.

[3] The readings were taken for outdoor conditions. Dr. Marchesi recorded this information only on the original record sheets in the case of Series B (PDT). Since only seven of these originals reached this Laboratory, no evaluation according to weather conditions can be made on the remaining four sheets, representing 40 runs.

The trends are not significant, but the data suggest that there may be a falling-off of scoring rate with the rise of humidity. The wider and more abrupt swings in scoring rate at the lower temperatures may be regarded as suggestive.

Table 2

METEOROLOGICAL CONDITIONS COMPARED WITH ESP SCORING RATE

TEMPERATURE					HYGROMETER			
Degrees Cent.	Runs	Dev.	Av. Score		Relative Humidity (per cent)	Runs	Dev.	Av. Score
+12	10	− 1	4.90		85	40	+15	5.38
+10	20	− 2	4.90	4.90	80	20	+12	5.60
+ 8	10	− 1	4.90		75	30	− 6	4.80
+ 6	50	+ 8	5.16		70	87	− 2	4.98
+ 4	30	+20	5.67	5.31	65	93	+36	5.39
+ 2	60	+16	5.27		60	30	+19	5.63
0	23	+17	5.74		55	14	+17	6.21
− 2	..	....	....	5.58				
− 4	20	+ 8	5.40					
− 6	14	+ 1	5.07		BAROMETER			
− 8	..	....	....	5.55				
−10	17	+16	5.94		mm. of Mercury	Runs	Dev.	Av. Score
−12	10	− 5	4.50					
−14	..	....	....	5.07	770	100	+19	5.19
−16	20	+ 7	5.35		765	70	+36	5.51
−18	20	− 3	4.85	5.23	760	64	+12	5.19
−20	10	+10	6.00		755	50	+21	5.42
					750	30	+ 3	5.10

DISCUSSION

By the customary standards of evaluation, the 354 runs carried out with Dr. Marchesi as subject gave a significant set of results. The CR has a probability of .01. On the basis of this CR it appears safe to conclude that ESP occurred in the production of the results.

It may be of some interest also to pool the results of Dr. Marchesi's present series with those he obtained in 1939-40. When this is done, the total number of runs is 707 with a deviation of +112 which gives a CR of 2.10. If we include also the rest of the earlier Durham-Zagreb data (the work of the 10 Durham subjects who participated in the experiment by attempting to identify cards

in Zagreb), we have a total of 1,707 runs, with a deviation of +200. The CR is 2.42, which has a significant probability of .008.

The 4,000-mile distance between Durham and Zagreb is the greatest over which ESP has so far been tested, and the significant total results give added evidence that ESP is not limited by space. These experiments recognizably do not include a precise control on the effect of distance, but the fact that ESP does occur over so many miles gives proof certainly of the fact that such a distance is not an absolute barrier. It remains to be seen (if experimentation can be continued) whether Dr. Marchesi can do better with a set-up that is identical except that a short distance is used.

It is not possible to say with any assurance why there was no significant relation between the run salience and the segmental salience in the present experiment though this was the principal feature of the earlier work of Dr. Marchesi. But although they are insignificant in covariation, the run and the segment distributions show a striking similarity between their curves (see Figure 1).

The rank order correlation of these total deviations of run and segment for the three series is +.825, which, with such a small num-

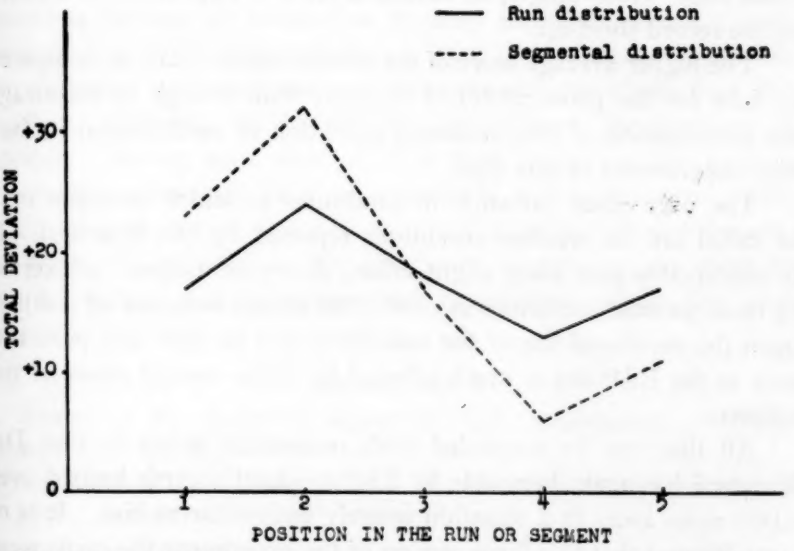

FIG. 1. The distribution of the total deviation within the run and within the segment for the DT and PDT tests combined.

ber of positions, can be only suggestive. This correlation is about the same as that given by the earlier Marchesi data, which also showed a marked paralleling of the run and segment distributions, the rank order correlation being +.90. In that case, however, the distribution curves were almost the exact mirror images of those shown here. The two experiments agree well in showing closeness of relationship between segment and run structuring.

Why the curves should reverse for the two experiments cannot be settled with finality. A possible lead might be found in the fact that Dr. Marchesi's procedure appears to have been different in the present experiments from his procedure in the earlier one. From his description of his participation in the present test (written before he had any knowledge of the results), it seems clear that he was led to take a psychologically opposite approach in making his way through the decks in the DT series because of the fact that the cards were standing on end. Instead of beginning with the card nearest to him in what would normally be considered the top of the deck, he reports that he went around, as it were, to the far side of the deck and, in his first call, attempted to identify the card farthest away from him. Accordingly, he recorded his DT calls from the bottom of the record sheet up.

The higher average score of the colored cards (5.31, as compared to 5.24 for the plain cards) is no more than enough to encourage the continuation of this incidental condition or modification in further experiments of this type.

The only other variation in conditions to which attention may be called are the weather conditions reported by Dr. Marchesi. It is conceivable that some slight effect, direct or indirect, is exerted by these weather conditions in view of the known influence of weather upon the emotional life of the individual and the fact that performance in the ESP test is much affected by subtle mental states of the subject.

All that can be concluded with reasonable safety is that Dr. Marchesi has again been able by ESP to identify cards located over 4,000 miles away in a situation entirely unfamiliar to him. It is of some interest that in a large section of the experiment the cards were not set up until a day after the calling, and that while this series is

not independently significant, the score average is the highest of the three series.

All the other observations are inconclusive in nature, but they add something, nevertheless, to the value of the experiment.

What, then, is the major importance of the present Marchesi experiment? The results are marginal in their significance and the incidental features of the experiment are lacking in significance. That which stands out as extraordinary is the simple fact that such an experimental performance can be done. First of all, it is the work of a rare individual merely to undertake such an experiment with the essential confidence and understanding. Dr. Marchesi is necessarily a thorough and devoted student of parapsychological research or he could hardly have had the requisite confidence, on the one hand, and the great patience for all the scientific detail, on the other.

In addition to this requisite attitude of the scientist, no little personal courage was required. Research of a parapsychological nature was not popular in Jugoslavia when these experiments were carried out. Difficulties were met with that we never encounter in our own country. In fact, these difficulties became insurmountable and the experiment had to be discontinued. When we consider what is required in the way of motivation to carry through such an experiment as this we may better appreciate some of the factors responsible for successful ESP performance. We are reminded of the Soal and Goldney series that ran on through the years of the bombing of London. Behind such work as this there is, to say the least, a tremendous earnestness and an irresistible will to discover. This factor of motivation may not be an essential one for all experiments, but there is inspiration in Dr. Marchesi's experiment for those of us who remain free to explore and experiment without fear and hardship.

References

1. Rhine, J. B. Terminal salience in ESP performance. *J. Parapsychol.*, 1941, **5**, 183-244.
2. Rhine, J. B., and Humphrey, B. M. A transoceanic ESP experiment. *J. Parapsychol.*, 1942, **6**, 52-74.

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TRIAL-BY-TRIAL GROUPING OF SUCCESS AND FAILURE IN PSI TESTS

By J. G. Pratt

ABSTRACT: Do ESP and PK occur in single flashes or in long stretches? To answer this question Dr. Pratt made a trial-by-trial study of hits in five ESP card series and two PK series which gave high scores. His purpose was to see whether the ability operated only instantaneously or whether there was enough clustering of hits to indicate a continuous relation between successes from trial to trial.

For comparison with the ESP and PK series, he also analyzed some records of visual perception tests in which ESP symbols were used under conditions of dim illumination.

None of the ESP or PK series showed enough clustering of hits to justify the hypothesis of a connection between trial-to-trial success. In other words there was no evidence that success in one trial in itself determines success in a neighboring trial. In the visual tests, however, the successes were found to be definitely grouped within the trial-by-trial sequence. Hence, psi seems to have occurred in quick flashes, and the visual perception to have been more of a sustained process.

These facts point the way to some interesting inquiries into the relationship between sensory perception and extrasensory perception.—Ed.

IT IS KNOWN that psi capacities do not operate with direct conscious control, evocable on demand. Instead there is a spontaneous or flash character to their working. Yet we also know that successful subjects have been able to demonstrate their ESP abilities by consistently scoring above "chance" through many experimental sessions.

These observations lead to the general question: Are the spontaneous operations of psi of variable duration so that they sometimes extend over a period of time or sequence of trials, or are they confined entirely to the individual trial? More specifically, would a trial-by-trial study of the distribution of successes in significant psi data show any continuity of hits, indicating that the subject had demonstrated his psi ability in "streaks of success," or would it show each hit to be independent of the others? The answer to this question was sought by making a study of some of the significant ESP and PK series which have been reported.

In order to compare the psi function with that of sensory per-

ception in regard to the degree of continuity shown, I also analyzed the results of some visual tests based upon ESP symbols exposed under dim light.

Method

The method adopted was that of counting the number of success loci in a sequence of trials and then evaluating the observed number in relation to the loci that would be expected on a purely random basis. A success locus is defined as an isolated hit or a group of hits coming on successive trials in the data. A statistical method for the evaluation of the frequency of such success loci in a sequence of alternatives (as when each trial must be either a hit or a miss) has been worked out by Stevens (**9**).

The problem with which we are concerned in this study is one having to do with the psychological nature of psi phenomena and not with the question of the occurrence of ESP and PK. We are interested, that is, in taking up data which are known to have yielded significant results leading to the conclusion that ESP or PK had been demonstrated and in finding out how the hits in such cases were distributed throughout the sequence of trials. The Stevens method is ideally suited to this particular problem, for it tells us how many success loci should occur in a sequence of trials when the actual run scores themselves are taken into account.

A simple illustration will help to make clear how the evaluation of the number of success loci bears directly upon the problem of whether the psi process works by grouping successes or is discontinuous from trial to trial. In the following two ESP runs, where *s* indicates a successful trial and *f* a failure, the scores for both are the same, 12 hits. The beginning of each success locus is marked by a vertical line.

$$|sfff|ssf|sfff|ssf|sf|sssff|ssff$$
$$f|sf|sff|sssssfffffff|sssssff$$

The former run contains seven success loci, a number which is very close to the expected average (6.72) for runs having a score of 12 hits out of 25 trials. In spite of the high score made on that run, there is no reason for supposing that there was any principle at work which led to the grouping of hits.

In the latter run, on the other hand, there are only four success loci for the same run score of 12 hits. Here the pattern is one of fewer success loci and therefore of longer groups than would be expected on a random basis. If this were found to hold true generally for the runs of a series, the results would show that the principle at work exhibited a continuity of success, sometimes spanning several trials in succession.

We know, of course, in working with data of ESP and PK tests, that a certain level of success is expected on a purely random basis. In this study we are concerned only with the nature of those successes over and beyond those which may be ascribed to chance. If the analysis is based upon a sufficient number of runs, a statistical study of the number of success loci in a series will show whether the process which gave the extrachance results was one which exhibited continuity or not.

Stevens' method for the evaluation of grouping has been described in more detail in the appendix.

Data

The records of five significant ESP series and of two outstanding PK experiments were analyzed in part or in full. The ESP series were as follows: the Riess Series (6); the Pearce-Pratt Distance Series (4); Pearce clairvoyance work without distance (3); the Soal and Goldney Series (8); and the Ownbey-Turner Telepathy Series (5). The PK data analyzed were from the Gibson Cup Series (1) and the Reeves High-Dice Series and the Reeves Low-Dice Series (2).

The sensory perception data analyzed were the results of some liminal vision tests based upon ESP symbols (7).

In some cases only a part of a series was utilized in the analysis. It is advisable as well as legitimate in an investigation such as this one to select not only the series to be analyzed but also the specific records to be studied within each one. Such selection, of course, must always be made without knowledge of how the analyses will turn out. Data which are barren from the point of view of evidence for the occurrence of psi phenomena would scarcely be of interest in research dealing with the way in which genuine hits occur. There

is no point, therefore, in working with insignificant experimental series. Furthermore, recent studies of position effects in psi test data have shown that there are often great differences in the level of success achieved between one point of the record structure and another. Since the present study is an investigation of the degree of grouping *in records showing a high general level of success,* it is appropriate to select for analysis the outstanding columns or runs from the records of specific experiments.

This is a preliminary report of an exploratory research. The results of all the analyses that have been done thus far are reported. It is being made at this time in the interest of encouraging a wider discussion of the problem and in the hope that other investigators may become interested in making similar analyses of their own data.

No broad generalization beyond the experimental series dealt with can be made at the present stage of the research. Nevertheless, the five ESP and the two PK series analyzed represent a wide variety of conditions and also a considerable range of score average. The following descriptions cover the essential facts of each series insofar as it was analyzed in the present study.

1. The Riess Series, the complete records of which have been published, consisted of 74 runs using the GESP condition over a distance of about one-quarter of a mile. The calls were timed at one-minute intervals. The average for the entire series was 18.24 hits per run, the highest on record.

2. The Pearce-Pratt Distance Series consisted of 74 runs done at distances of 100 and 250 yards. This was a clairvoyance experiment with the calls spaced at one-minute intervals. The average of the entire series was 7.53 hits per run.

3. For purposes of comparison, 51 runs which gave scores of 8 or higher were selected from Pearce's clairvoyance work done with the experimenter and the cards in the room with the subject. In these tests the calls were made at the rate of one trial every two or three seconds.

4. The Soal and Goldney data used here are those obtained from their subject, B.S., who got his most significant results with the GESP condition. His success, however, came largely in the form

of forward displacement, or hitting the next card in the sequence instead of the one upon which the sender was concentrating at the time. The full records of only 82 runs of this series are available at present in the Parapsychology Laboratory, and these were analyzed for grouping of hits in forward displacement.

5. The Ownbey-Turner Telepathy Series consisted of only 13 runs made over a distance of approximately 250 miles. The calls were spaced at intervals of three or five minutes. The entire series is outstanding for the unusually high scores made at the start and for the sharp decline in scoring as the series progressed.

6. The Reeves High-Dice Series and the Reeves Low-Dice Series, the first of the two PK experiments studied, were comparable to the ESP work in that each trial presented the simple alternative of a success or a failure. Earlier reports based on this work have shown that the most significant scoring occurred in the first column of the three-column set, and therefore I decided to confine the study of the grouping of successes to the first column of each set, which in both series combined totaled 299 columns or runs.

7. In the Gibson Cup PK Series, six dice were thrown at a time for a certain target face, and the number of hits per throw was entered on the record. Since each trial score was not simply either a hit or a miss, the Stevens method was not applicable to these data. But the question of whether the subject's successful throws tended to come together was also relevant here, and it could be answered by breaking the data up into pairs of consecutive trials and correlating the scores within the pairs. This analysis was made on the data of the first column of the set for the unwitnessed work of the principal subject, L.H.G. The most significant scoring of the series occurred in these trials. The first columns of 113 sets from all her record pages containing only unwitnessed work were included in the analysis.

8. The sensory perception (SP) data analyzed were a part of the research done by B. M. Smith for the doctoral dissertation. In his tests, ESP symbols were presented at the approximate rate of one every three seconds. The illumination was adjusted so that each subject averaged about 50 per cent correct in naming the symbols.

I selected for analysis the records of eight subjects who were tested under normal conditions (after taking a disguised dosage of lactose), a total of 145 runs.[1]

Results

The application of the Stevens test in the five ESP series and in the high-dice series and low-dice series gave insignificant CR's in every instance, showing that the manner in which the hits were distributed throughout the sequence of individual trials was not unlike what would be expected on a random basis. In other words, in spite of the fact that the subjects scored significantly above "chance" in all these series, there is no evidence of any extrachance grouping of successes from trial to trial within the run. The internal sequence of hits within each run is not distinguishable from the distribution expected in a purely random arrangement of hits and misses in the ratio indicated by the run scores.

The analysis of the Smith SP data, however, gave a CR of 5.32, a highly significant result which indicated a smaller number of success loci within the data than would be expected on a chance basis. This means that, given the number of hits which were actually scored, the subjects tended to group them into longer sequences than a random distribution would have given. Grouping of successes therefore occurred in the SP data, but not in any of the ESP or PK series which were analyzed by the same procedure.

The results of the analysis of the Gibson Cup PK Series agree with those found for the other ESP and PK results: there was no evidence of any grouping of successes on consecutive trials. The correlation obtained, based on the scores of 339 pairs of throws, was −.04, which is insignificant.

These results are all summarized in Table 1 where the significance of the evaluation of each series by the Stevens method is given in terms of the CR shown in the right-hand column. A plus sign with a CR shows that there are more loci of success within that series than the average number expected by chance. Conversely, the minus sign shows that the number of loci within the data where

[1] Only runs which gave scores of 8 or higher were included, as I wished to be reasonably sure that visual perception was operating.

hits occurred was less than mean chance expectation. All of the CR's for the psi test results are well within the range of chance variation, while the CR for the SP results is so large that the degree of grouping represented would be expected by chance only one time in approximately one million such series.

Table 1

GROUPING OF SUCCESSES IN DATA ANALYZED BY STEVENS' METHOD

	Runs	Groups Observed	Groups Expected	Dev.	SD	CR
ESP Series						
Riess	74	372	374.16	− 2.16	8.29	(−) .26
Pearce-Pratt	74	386	374.08	+11.92	8.80	(+)1.38
Pearce Clairvoyance	51	326	319.68	+ 6.32	8.39	(+) .75
Soal and Goldney	82	412	413.75	− 1.75	8.90	(−) .20
Ownbey-Turner	13	66	61.32	+ 4.68	3.22	(−)1.45
PK Series*						
Reeves, HD and LD	299	981	963.33	+17.67	14.14	(+)1.25
SP Series						
Smith Data	145	838	914.36	−76.36	14.34	(−)5.32

* The Gibson Cup Series could not be analyzed by the Stevens method. The six-trial columns selected for study were divided into 339 pairs of consecutive throws. The product-moment correlation of odd and even scores within the pairs was —.04.

DISCUSSION

As far as the present analyses go, they show that, given the actual number of hits scored, the degree of grouping found in psi test results is no more than one would expect on a random basis. The expected number of loci of success within a run varies with the total number of hits made. The Stevens method, however, takes account of the run scores and measures the degree of grouping among the hits obtained, whatever their number.

Since this investigation has uncovered no evidence that grouping occurs in psi test results, the conclusion indicated is that the flash character of ESP and PK is restricted to the individual trial. There is no continuity of the psi process which leads to a linking of hits through two or more consecutive trials in the sequence as presented in a quantitative, experimental test situation.

At the same time the results of the single SP series point unmistakably to a trial-to-trial continuity of the visual process.

It may be that we have here some evidence of a fundamental distinction between the operation of psi capacities and of visual perception. If the distinction proves through further research to be a basic one, that fact would seem to be one of considerable importance for our understanding of the psychological nature of psi phenomena.

It must be emphasized again, however, that the number of series analyzed, particularly in the SP field, is too small for sweeping generalizations. This apparent distinction between psi phenomena and SP can properly be discussed more fully at this time, therefore, only if we make due reservations for the manner in which the factual picture might be changed through further research.

One question which arises regarding this apparent difference between psi and SP is whether the degree of grouping found in each series is uniform throughout the entire score range covered. It has already been said that the Stevens method takes account of the variation in run scores. It does not necessarily follow, however, that the degree of grouping in a series as a whole must be equally characteristic of all levels of scoring success. It is possible, for example, that a subject might show no grouping tendency in the series as a whole, but that a further analysis in terms of different levels of success would indicate that his hits were related to one another within the runs of a particular range of scores.

The question raised here is one which is easily answered in the course of making the analyses. As may be seen from the appendix, the Stevens test of grouping must be applied to each run of a series in terms of the score made. In actual practice this means that we begin an analysis by grouping runs according to their scores. In ESP runs of 25 trials, for example, we then proceed to work out the evaluation for the runs of each score level by multiplying the appropriate expectation and variance (as shown in the table in the Appendix) by the correct number of runs. It is a simple matter to complete the evaluation for grouping at each score level and to see whether there is any particular range of run scores for which the degree of grouping does not conform to the results found for the entire series.

This evaluation of grouping at different score levels was carried out for all the ESP and PK series analyzed. It was found that four

series showed a random degree of grouping throughout their entire score range. In two instances, however, the results were suggestive of a nonrandom relationship in the direction of isolation of successes, or negative grouping. One of these was the Pearce-Pratt Distance Series. When these runs were divided into two approximately equal groups in terms of the run scores, it was found that those runs with scores of 9 to 13, the upper score range, gave 248 loci of success where only 233.52 were expected. This amounted to 14.48 more loci than the number expected. The SD is 7.31, and the CR, 1.95, which is barely suggestive.

The other interesting (though again only suggestive) exception to the rule of random grouping was found in the high-dice and low-dice PK data. There were, it will be recalled, 299 runs involved. Runs of five and below were combined into one group and those of six and above in another, as this gave the most nearly equal division of the data. In the low-scoring group, the grouping of hits was purely random. In the higher score range, however, there were 573 loci of success, 30.33 more than the average number expected. In this instance, the SD is 10.94, giving a CR of 2.77, which is again in the direction of a nonrandom degree of isolation of hits.

The SP series gave uniform grouping of successes throughout the entire range of its run scores.

The two cases showing a tendency toward a negative grouping of hits in psi tests are, I repeat, of a purely suggestive character. It is interesting to see, however, that they both occur in the higher score range of the series and that both deviations are in the direction of isolation of successes. Theoretically, it is conceivable that successes might be *negatively* related to one another, leading to more than the expected number of success loci and to groups that are too small to explain on a chance basis. Further research is necessary to show whether such a negative grouping does occur in psi test data.

For the time being, the results of this investigation can be interpreted only as showing that the psi function demonstrated in the card-calling and dice-throwing tests analyzed produced only a ran-

dom degree of grouping of hits, whereas visual perception near the limen was found to show significant grouping in the one situation studied. This difference between psi and SP cannot be explained in terms of superficial differences in the experimental conditions. It is true that some of the ESP series were done at a rate which was much slower than that used in the SP work, and it is not surprising that these slow ESP tests did not show grouping. The two ESP series which did not involve the distance factor, however, were done at a rate as rapid as that of the SP work. These were the Pearce clairvoyance data, and the Soal and Goldney tests. Also, the successive trials in the high-dice and low-dice tests were very probably no more widely spaced in time than were the visual perception trials.

If psi processes and SP are basically different with respect to the continuity of operation, it seems likely that the explanation will be found to be associated with the fact that the psi process is essentially an unconscious one. In an ESP test the subject is unable to distinguish his successful trials from those which are failures, whereas sensory perception normally occurs in such a manner that a person may have an introspective knowledge of when he is right. The sequence of calls in an ESP test is only partly the result of the operation of psi capacities. In addition to the ESP factor itself, there are complex rational associations which break into or interrupt the sequence of genuine ESP calls in an unpredictable manner.

The most marked differences between psi phenomena and sensory perception may arise from the fact that the former are relatively more central within the mental system, and the latter relatively more peripheral. Sensory perception under near-liminal conditions is a function which varies with a number of factors, including the conditions of the sensory mechanism itself, the intensity of stimulation, the stimulus object, and the general physiological state of the individual. At the same time other factors of a more purely psychological nature affect the process. As an individual takes a test in near-liminal vision, the relevant physical, physiological and psychological conditions vary from time to time. When the particular combination of conditions becomes favorable for seeing the stimulus, the success is likely to carry over through two or more trials in a rapid-

fire test, and a sequence of hits is made. On the other hand, when the situation becomes unfavorable, two or more failures in succession are generally expected.

Many of the psychological factors which affect visual perception also influence the level of success in an ESP or PK test. The factor of motivation, for example, seems to be even more important for the demonstration of psi abilities than it is for seeing an object in dim light. Other psychological factors, probably best described in terms of the concepts of inhibition and repression, appear to affect the psi process to a far greater degree than they do that of SP.

On theoretical grounds, grouping of hits seems less likely to occur in PK tests than in ESP data. In tests with dice the subject's success would appear to depend largely upon the varying and highly complex physical situation presented by the rolling dice. On some trials a relatively strong PK influence might result in a miss because the dice themselves were unfavorably situated. This physical factor, which may set a limit on the level of success and the degree of grouping which are possible in PK tests, is not present in the ESP situation. To throw further light upon the problem raised in this study, therefore, the need for more research into the relation between ESP and SP is particularly urgent.

The absence of grouping in the two PK series analyzed has a direct bearing on the problem of the nature of position effects. The question has recently been raised whether position effects in pooled data reflect trends of scoring in the individual columns, or whether they are a statistical effect of pooling individual columns (1). The present study indicates that the latter is the case.

The PK data analyzed were selected because they showed highly significant position effects. In the Gibson Cup Series, there was a striking decline in scoring between the first and the last (sixth) trial positions in the column. If each individual column were a small exact copy of the pooled data, a high positive correlation would be expected when each of the 113 columns was divided into three pairs of consecutive trials and the scores on the odd and even trials were correlated for the resulting 339 pairs. This follows from the fact that the first two trials in each column would always both be

high in score; the middle pair, both intermediate; and the last pair, low. Of course, variations in scoring pattern within the individual columns certainly did occur, and no such perfect correlation could be anticipated from the analysis. However, any tendency for the individual columns to reflect the pattern of decline in scoring found in the pooled data would operate to produce a positive correlation. The fact that no correlation of any significance was found (−.04) shows that there was no pattern of scoring in the individual trials such as that which emerged when all of the columns were pooled.

In the high-dice and low-dice series, the pooled first column of the set contained significant position effects in the form of a U-curve distribution of successes. If a similar pattern of scoring were present in the individual columns, we would expect to find an unusually large number of cases in which groups of successes occurred near the beginning and the end of the run. Stevens' method applied to these data gave no evidence of grouping. This fact indicates, again, that the position effects were not present as an actual scoring trend in the sequence of individual trials, but belonged only to the pooled data.

Neither the statistical nor the psychological significance of position effects is reduced in the least by the discovery that they were a result of the method of analysis used. That position effects found in the pooled data reflect actual trends in scoring is a logical assumption to make, but these findings point to a different origin. The present study therefore serves to bring the problem of position effects into sharper focus. The presence of terminal salience in the pooled column appears to indicate only that it was *easier* for the subject to demonstrate his psi capacities on the trials which occupied positions near the beginning and end of the structured sequence of trials. The crux of the problem is to find what factor or factors cause the probability of making a genuine psi response to change in a lawful manner as the subject progresses from trial to trial through a test which presents a series of uniform, organized tasks.

Concluding Remarks

This investigation has not produced any evidence that ESP and PK successes occur in larger groups than would be expected on a

random basis *when the run scores themselves are taken into account.* The subject does, however, exercise some control over his psi capacities. In the laboratory situation he takes tests by appointment, the arrangement frequently having been made days or weeks in advance. He usually works at the request of the experimenter and not merely according to his own whims. He can score above "chance" or below "chance" as the experimenter may direct. But his control is definitely limited. All the past accumulation of experimental evidence indicates that the subject is not conscious of success or failure in a psi test. As far as the present study has gone, the genuine psi responses within the trial sequence were found to have occurred independently of one another. In spite of the fact that successful subjects can turn the process on or off and use it to get positive or negative deviations at will, the trial-by-trial sequence of events during the operation appears to be beyond the subject's power to regulate.

APPENDIX

Stevens' Method for the Evaluation of Grouping in a Sequence of Alternatives

Given a sequence of simple alternatives, it is desired to evaluate the probability that a random arrangement of the given series would yield a number of groups (an unbroken succession of one or more of the same alternative being defined as a group) as large as, or as small as, the number actually observed.

Consider the problem as related to the distribution of hits within a run of 25 calls. Let S = no. of successes, F = no. of failures, and Y = no. of groups of one or more successes in the run. Then

$$\text{Expected value of } Y = \frac{(S)\ (F+1)}{(S+F)} = \frac{(S)\ (F+1)}{25}$$

$$\text{Variance of } Y = \frac{(S)\ (S-1)\ (F+1)\ (F)}{(S+F)^2\ (S+F-1)}$$

$$= \frac{(S)\ (S-1)\ (F+1)\ (F)}{15000}$$

If the results of a number of sets of data (runs) are combined, the sum of the number of groups of successes has an expectation and variance found respectively by summing the expectations and variances for each set of data. The SD of a series is, of course, the square root of the sum of the variances for the individual runs.

The above formulas are stated in terms of the standard run of 25 trials because of the wide use made of this particular length of trial sequence in ESP research. The formulas may, however, be applied to any length of sequence of alternatives. In the application of the method to the Soal and Goldney displacement data, for example, only 24 forward displacement trials are included in each run and therefore the value of $(S + F) = 24$.

It should be noted that no assumption is made as to the probability of success on a single trial, the *observed* proportion of successes and failures in each run being taken as the given data.

The following table of values for applying the Stevens method to data based upon the standard 25-card run is published here in the belief that other research workers may find it useful.

TABLE OF EXPECTED VALUE AND VARIANCE OF THE NUMBER OF GROUPS OF SUCCESSES (SUCCESS LOCI) WITHIN STANDARD ESP RUNS WITH SCORES OF ZERO TO TWENTY-FIVE HITS

ESP Run Score	*Number of Groups Expected*	*Variance of the Number of Groups at Each Score Level*
0	0	0
1	1.00	0
2	1.92	.0736
3	2.76	.2024
4	3.52	.3696
5	4.20	.5600
6	4.80	.7600
7	5.32	.9576
8	5.76	1.1424
9	6.12	1.3056
10	6.40	1.4400
11	6.60	1.5400
12	6.72	1.6016
13	6.76	1.6224
14	6.72	1.6016
15	6.60	1.5400
16	6.40	1.4400
17	6.12	1.3056
18	5.76	1.1424
19	5.32	.9576
20	4.80	.7600
21	4.20	.5600
22	3.52	.3696
23	2.76	.2024
24	1.92	.0736
25	1.00	0

REFERENCES

1. PRATT, J. G. Lawfulness of position effects in the Gibson Cup PK Series. *J. Parapsychol.*, 1946, **10**, 243-68.

2. ———. Rhythms of success in PK test data. *J. Parapsychol.*, 1947, **11**, 90-110.
3. RHINE, J. B. *Extrasensory Perception.* Boston: Bruce Humphries, 1934.
4. ———. Some basic experiments in extrasensory perception—a background. *J. Parapsychol.*, 1937, **1**, 70-80.
5. RHINE, J. B., PRATT, J. G., STUART, C. E., SMITH, B. M., and GREENWOOD, J. A. *Extrasensory Perception After Sixty Years.* New York: Henry Holt, 1940.
6. RIESS, B. F. Further data from a case of high scores in card-guessing. *J. Parapsychol.*, 1939, **3**, 79-84.
7. SMITH, B. M. *Effects of Sodium Amytal on Visual Perception of Forms at the Limen.* Ph.D. Dissertation (1947), Duke University Library. [Unpublished MS.]
8. SOAL, S. G., and GOLDNEY, K. M. Experiments in precognitive telepathy. *Proc. Soc. psych. Res.*, Lond., 1943, **47**, 121-50.
9. STEVENS, W. L. Distribution of groups in a sequence of alternatives. *Ann. Eugen.*, 1939, **9**, 10-17.

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COMPARISON OF TWO DISTANCES IN PK TESTS

By Carroll Blue Nash and Alice Richards

ABSTRACT: Scores made by subjects in PK tests in which they were three feet away from the dice were compared with the scores they obtained when they were 30 feet away. Forty-eight college students were tested individually with a procedure in which 24 dice were thrown at a time by gravity. This procedure was also combined with a comparison of scores made when a small reward was offered and scores made when no reward was offered. The number of successes obtained in the total of 1,536 runs made under all conditions was greater than would be expected by "chance" but it was below the level of significance. The runs made at the distance of 30 feet, however, were marginally significant. The no-reward tests gave higher scores than did the tests in which a reward was offered.

Instead of the scoring declines usually found in the course of PK experiments, significant inclines appeared in the various subdivisions of this experiment; for example, the scores obtained in the second chronological half of the series were significantly higher than those obtained in the first half. In view of the consistency of these inclines, the writers suggest that the experimenter instead of the subjects may have been the one who was actually influencing the dice in these PK tests.

Miss Alice Richards was a senior student in zoology at Washington College at the time this experiment was carried out.—Ed.

In the report of an earlier experiment (3) by one of us (C.B.N.), a comparison of the results of PK tests carried out at two distances showed no appreciable difference between the scores obtained when the subject was three feet away from the dice and those obtained when he was 30 feet away. In that experiment, however, the experimenter stood at the shorter of the two distances throughout the entire series. It was considered possible, therefore, that he might have influenced the dice and produced the results that were obtained.

The present experiment, carried out between January and May, 1947, was performed in order to clear up the uncertainty concerning the relationship between PK and distance. The PK tests were made at the same two distances as were used in the previous experiment (three feet and 30 feet). In the present series, however, the experimenter (A.R.) remained near the subject during the tests made at

the two distances so that there was no one closer to the dice than the subject.

In addition to the comparison of the PK effect at two distances, the experiment was also designed to provide a comparison between trials in which a small reward (two movie tickets) was offered for high scores and those in which no reward was promised.

Twenty-four dice, 15 mm. in diameter, with excavated spots were used in the experiment. All 24 were thrown at a time. Before each roll, the dice were placed in an open-topped container which was hinged to a three-foot-long inclined board covered with corrugated paper. A cord attached to the container, when pulled by the subject, overturned it, permitting the dice to roll down the inclined board into a cloth-lined, open-topped box in which the lower end of the board rested.

Forty-eight students at Washington College were the subjects of the experiment. Each subject was tested in two sessions which were spaced from one day to two months apart. The session consisted of two "sets" of trials which were completed in one sitting. The set consisted of eight runs, and the run consisted of a single roll of the 24 dice. The subjects were divided into four groups so that the conditions of distance and reward, as well as the use of the six target faces of the die, could be systematically varied. The sequence of procedures followed by the four groups of subjects is shown in the following table:

Group	Target Face	Session 1		Session 2	
		Set 1	Set 2	Set 1	Set 2
1	1, 5, 3	3′, Rew.	30′, No. rew.	3′, No. rew.	30′, Rew.
2	2, 6, 4	30′, Rew.	3′, No. rew.	30′, No. rew.	3′, Rew.
3	3, 1, 5	3′, No. rew.	30′, Rew.	3′, Rew.	30′, No. rew.
4	4, 2, 6	30′, No. rew.	3′, Rew.	30′, Rew.	3′, No. rew.

The first four subjects were assigned in their respective order to the four groups. The first subject took die face one; the second, die face two; the third, die face three; and the fourth, die face four. The second four subjects were similarly assigned to the four groups and target faces five, six, one, and two; and so on for the rest of the subjects. This distribution of subjects resulted in die faces one,

three, and five being the targets for Groups 1 and 3, and die faces two, four, and six being the targets for Groups 2 and 4. Thus the assignment of targets was designed to provide an equal number of trials for each of the six faces of the die, and also an equal number of trials for each face of the die in the tests made under the different conditions of distance and reward. The practice of assigning subjects to the four groups was followed throughout the experiment with two minor exceptions: one subject was accidentally assigned to Group 1 instead of to Group 3, so that Group 1 has 13 subjects while Group 3 has only 11; and in Group 4 one subject was tested in both sessions under the 30-foot-reward and 3-foot-no-reward conditions.

Before each set, the subject was told whether or not the reward applied to the set. The experimenter (A.R.) and the subject remained together at the same distance from the dice throughout the session. Following each run, they both counted the hits. The eight runs for the particular distance and condition of reward were completed before the second set was done. The two sets were done in one session.

A separate record page was used for each of the subject's two sessions. As the session consisted of two sets, the page contained score spaces for the eight runs of each of the two sets. The hits for each of the eight runs were recorded vertically in two columns, the first four runs being recorded in the left column and the second four runs in the right column. (See Figure 1.) The hits for the second set were recorded on the same page below the hits for the first set.

At the request of the Parapsychology Laboratory of Duke University, a clairvoyance drawings test was given after each set of PK tests in order to see if there was any relationship between the form quality of the drawings (expansiveness or compressiveness) and the PK scores made by the same subject. Previous studies (2) have shown that the form quality of drawings made in ESP tests bear a relationship to the ESP scores obtained in the test. It was considered possible that the form quality of the drawings might also reveal something of the subjects' attitudes toward the PK test. Thus, after each set of these dice tests, the subject was handed a sealed

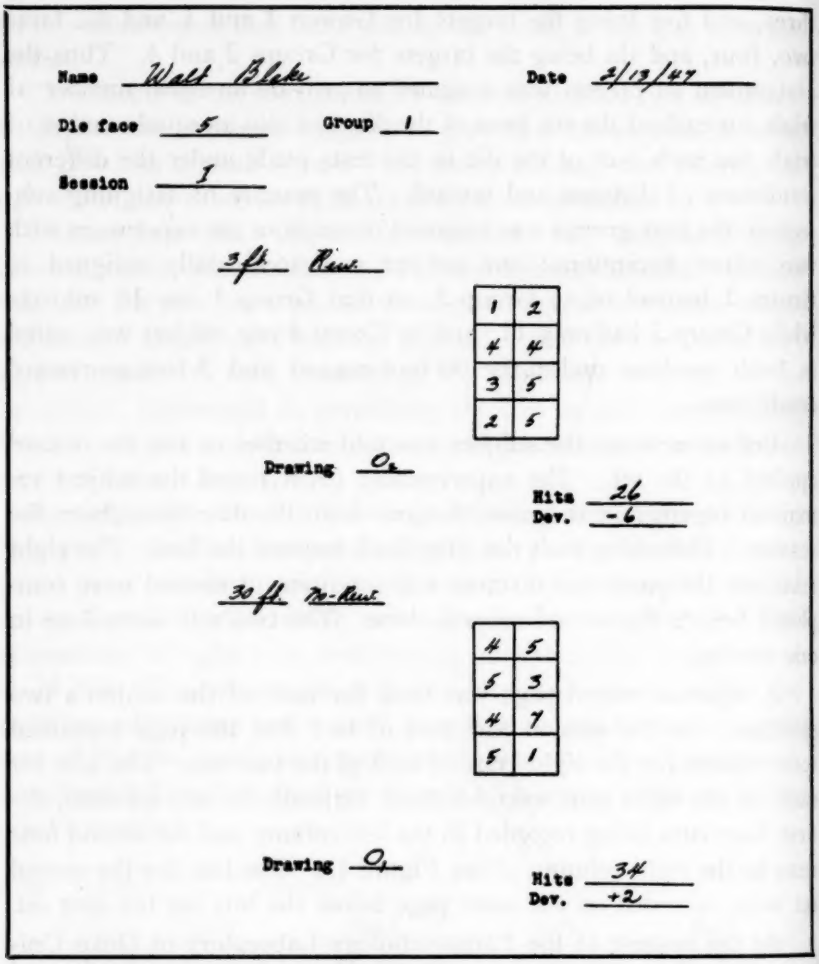

FIG. 1. Copy of a record sheet for a subject in Group 1. The number of hits for each throw of 24 dice was entered in each cell in the 8-cell sets. After each set of PK, the code letter for the subject's clairvoyance drawing was recorded.

envelope containing a picture and was asked to draw his impressions of the enclosed picture. Since each subject did a total of four sets of PK tests (two sets each session), each one also made a set of four clairvoyance drawings (two in each session). The drawings and stimulus pictures were sent to the Parapsychology Laboratory, where two independent judges matched them by the preferential

matching method (5), and one judge rated them for expansiveness of form quality.

All PK records were sent to the Parapsychology Laboratory, where the analyses and computations were independently checked.

Results

In the entire experiment, regardless of conditions, a total of 1,536 runs were made. The number of hits expected by chance is 6,144, while actually a total of 6,302 hits were obtained. The deviation is therefore +158 and the CR is 2.21 (P = .014), which is in the suggestive range although it is slightly below the criterion of significance (2.33).

Distance Comparison

The results obtained with the subject at a distance of 30 feet from the dice show a positive deviation of 119, while the same number of runs (768) made at 3 feet give a deviation of +39. The CR of the difference between the two conditions is only 1.12, which is not significant. The CR for the tests at 30 feet is marginally significant: 2.35 (P = .01). (See Table 1.)

Table 1

General Summary of Results

	3 Feet			30 Feet			Total		
	Runs	Dev.	CR	Runs	Dev.	CR	Runs	Dev.	CR
No Reward.....	376	+40	1.13	392	+ 63	1.74	768	+103	2.04
Reward.....	392	− 1	.03	376	+ 56	1.58	768	+ 55	1.09
Total.....	768	+39	.77	768	+119	2.35	1536	+158	2.21

Reward vs. No Reward

The results of the no-reward tests are higher than the results of the reward tests. The former have a total deviation of +103 in the 768 runs, as compared with a deviation of +55 in the same number of runs made under the reward conditions. The CR of the difference, however, is only .67 and is not significant. (See Table 1.)

Individual Target Faces

When the results were compiled for each of the six target faces, it was found that a positive deviation was obtained on all faces except the four-face and the one-face. The deviation on the former was zero, while a negative deviation of 23 was obtained when ones were the target. The deviation for each of the faces is given below:

Target Face	1	2	3	4	5	6
Runs	256	256	256	256	256	256
Dev.	−23	+28	+17	0	+81	+55

Since all faces served as target an equal number of times in the experiment, the hypothesis of biased dice cannot account for the results of the experiment.

Position Effects

As a rule, experimenters have reported that as a given unit of a PK experiment progressed there was a tendency for scoring to decline. This has been generally true of the distribution of scores on the record sheet for both the set and page. When the present experiment was analyzed for position effects, however, it was found that instead of a decline in scoring, a strong incline was most common.

Chronological Distribution. In analyzing the data for the chronological distribution of hits, the experiment was divided into two equal parts, one representing the work carried out in the first half (January 9 to March 13), and the other representing the work carried out in the second half (March 14 to May 6). The results of this division may be seen below.

	Runs	Dev.	CR	CR_d
First half	768	— 39	.77	3.30
Second half	768	+197	3.90	

The first half of the experiment was negative (−39) while the last half was significantly positive (+197), as its CR of 3.90 attests. The CR of the difference between the two halves (3.30) is likewise significant, having a P of .0005. Even when this probability is corrected for the direction of the difference it is still significant (.001).

Session and Set Distributions. It will be recalled that each subject was tested in two sessions and that at each session he completed two sets of eight runs (or throws) each. It was found in analyzing the data that more hits were made in the first session (+110) than in the second (+48), but this decline effect is not significant. On the other hand, between the two sets done in a session, an incline was found. Set 1 has a deviation of only +20, while Set 2 has one of +138. The difference between the two is not significant, but when Set 2 is considered alone, its CR is 2.73. Table 2 gives the results by sets for each session.

Table 2

RESULTS FOR THE TWO SETS AND THE TWO SESSIONS SEPARATELY

	SET 1			SET 2			TOTAL		
	Runs	Dev.	CR	Runs	Dev.	CR	Runs	Dev.	CR
Session 1.....	384	+33	.92	384	+ 77	2.15	768	+110	2.18
Session 2.....	384	−13	.36	384	+ 61	1.71	768	+ 48	.95
Total.....	768	+20	.40	768	+138	2.73	1536	+158	2.21

We were interested in seeing the relation of set scoring to the chronological periods of the experiment. Therefore we made such an analysis, the results of which may be seen in Table 3. Most of the scoring occurred in Set 2 during the last half of the experiment. This section of data alone has a CR of 3.63. The lowest rate of scoring occurred in Set 1 during the first half of the experiment. In both halves, however, there was an incline in scoring from the

Table 3

COMPARISON OF SETS WITHIN THE TWO CHRONOLOGICAL PERIODS OF THE EXPERIMENT

	SET 1				SET 2			
	Runs	Dev.	CR	CRd	Runs	Dev.	CR	CRd
First Half....	384	−47	1.31	2.25	384	+ 8	.22	2.41
Second Half..	384	+67	1.87		384	+130	3.63	

first to the second set; and in both sets, an incline from the first to the second half.

Column Distribution. Each subject completed 16 runs at each session. The record sheet was arranged so that there were four columns of four runs each, the two columns at the top of the page representing Set 1, and the two at the bottom, Set 2. Table 4 gives the results of an analysis for position effects by columns. It shows the results according to condition of distance, condition of reward, and position of column.

Table 4

COMPARISON OF RESULTS ON FIRST AND SECOND COLUMNS IN THE SET FOR THE VARIOUS CONDITIONS OF DISTANCE AND REWARD

	3 Feet				30 Feet				Total			
	First Column		Second Column		First Column		Second Column		First Column		Second Column	
	Runs	Dev.	Runs	Dev.	Runs	Dev.	Runs	Dev.	Runs	Dev.	Runs	Dev.
No Reward..	188	−28	188	+68	196	+27	196	+36	384	− 1	384	+104
Reward.....	196	−25	196	+24	188	+40	188	+16	384	+15	384	+ 40
Total...	384	−53	384	+92	384	+67	384	+52	768	+14	768	+144
CRd...	2.87				.30				1.82			

Again there is an incline, with most of the hits falling in the second column of the set. The CR of the difference between the two columns pooled (1.82) is not significant. There is a significant difference, however, between the two columns when only the runs done under the condition of three feet are considered. The CR of the difference is 2.87.

A further comparison of columns was made according to set and session. (See Table 5.) This analysis shows that most of the hits were made in the second column of Set 2. The CR of the difference between columns one and two in Set 2 is 2.53 and is significant. The greatest incline in scoring between columns occurred in the second set of the first session. The CR of the difference here between Column 1 and Column 2 is 2.38.

Table 5

COMPARISON OF FIRST AND SECOND COLUMNS IN THE SET FOR THE TWO SETS IN THE TWO SESSIONS

	Set 1					Set 2					Total				
	First Column		Second Column		CR_d	First Column		Second Column		CR_d	First Column		Second Column		CR_d
	Runs	Dev.	Runs	Dev.		Runs	Dev.	Runs	Dev.		Runs	Dev.	Runs	Dev.	
Session 1....	192	+12	192	+21	.25	192	−4	192	+ 81	2.38	384	+ 8	384	+102	1.86
Session 2....	192	−3	192	−10	.20	192	+9	192	+ 52	1.20	384	+ 6	384	+ 42	.71
Total...	384	+ 9	384	+11	.04	384	+5	384	+133	2.53	768	+14	768	+144	1.82

QD's (Quarter Distributions). The total set QD for all the data pooled is not a typical one. Most of the hits are found in the upper right quarter, and the fewest are found in the upper left-hand quarter. This distribution is due to the data from the three-foot tests since those for 30 feet show a typical QD with most of the hits falling in the upper left-hand quarter and the fewest in the lower right. The QD's are shown below.

3 Feet

192 −43	192 +46
192 −10	192 +46

$CR_{d(1-4)} = (-)2.49$

30 Feet

192 +46	192 +34
192 +21	192 +18

$CR_{d(1-4)} = .78$

Total

384 +3	384 +80
384 +11	384 +64

$CR_{d(1-4)} = (-)1.21$

When the QD analysis is applied to the page as a whole, the result is a complete reversal of the typical QD, with a CR of the

384 +9	384 +11
384 +5	384 +133

$CR_{d(1-4)} = (-)2.45$

difference between the first and fourth quarters of (−)2.45. With correction for direction of the difference, this CR is not significant (P = .014).

Incidental Findings

The ESP results of the clairvoyance drawings tests were insignificant, with a positive deviation of 13 for the total of 48 sets of four drawings each. When the drawings were separated, on the basis of form quality alone, into expansive and compressive groups, the following results were obtained:

	N	Dev.	Mean Score
Expansive subjects	4	—15	36.25
Compressive subjects	44	+28	40.64

(The mean score expected by chance is 40.00.) Neither group of subjects gave significant results, nor is the difference between the two significant ("Student's" t of the difference = 1.25, with 46 d.f.).

When the PK scores are considered separately for the subjects who made expansive drawings and for those whose drawings were rated compressive, the following distribution of scores was found:

	N	PK Runs	Dev.	Av. Run Score
Expansive subjects	4	128	+ 13	4.101
Compressive subjects	44	1408	+145	4.103

The results of this separation show no appreciable difference between the two groups of subjects. Neither are the results of either group independently significant.

One other incidental analysis was made at the completion of the experiment. The intelligence ratings of the subjects were obtained from the Higher Examination of the Otis Self-Administering Tests of Mental Ability, and these ratings were compared with the total PK results obtained by each subject. There was no evidence of a relation between intelligence and PK ability. (A Pearson's product-moment correlation of the two tests gave a coefficient of −.12 with 48 cases involved.)

Discussion

Counterhypotheses

The design of this experiment takes care of the question of imperfect dice. Since each die face served as target for an equal number of throws, any effect of dice bias was neutralized. Likewise, the mechanical method of rolling the dice eliminated the problem of skilled throwing. In the matter of errors of observation in recording, we have to rely, as does most research in any laboratory, upon the accuracy and efficiency of the trained observer. A.R. was responsible for the reading of the dice and the recording. The subject, of course, was always present as a secondary observer, and the score was called aloud before being recorded. The fact that the latter half of the experimental period, the one in which the experimenter naturally had more experience, gives much the better results, reduces the likelihood that the effects are due to recording errors. Also, the most significant findings had to do with internal differences rather than with total deviations; these would require a lawfully persistent differential in accuracy of observational recording, and that appears still more improbable. As in all research, however, the check on the human factors involved in individual observation lies in experimental confirmation.

The hypothesis of chance is not conclusively eliminated by the CR of the total deviation. The probability of .014 is slightly under the criterion of significance. It is true that tests at 30 feet did give a just significant total deviation with a CR of 2.35. The second half of the experiment, chronologically, gives a CR of 3.90, which is quite significant, and the difference between the two chronological periods is likewise significant. These results, together with other significant relations found in the analyses, justify the conclusion that we are not dealing with a chance distribution of scores. It seems highly probable, therefore, that PK was operating and that we may take the comparison of the two distances seriously.

Distance

While we cannot say that the results of the comparison of 3- and 30-foot distances gave a conclusive difference, it is noteworthy that the scoring rate is higher at the larger distance, high enough to be

independently significant if considered by itself. These findings confirm the earlier work reported by Nash and support the view that distances of the order under test do not show the effect on the scoring rate of the subject that would be expected if PK followed the laws known to govern physical forces.

Effect of Reward

The no-reward tests gave a total deviation almost twice as great as that given by the reward tests. No other PK research on the effect of rewards has been done, but there have been several investigations of reward effects in ESP experiments. When rewarded trials have been compared with non-rewarded trials made in the same session no appreciable difference has been found. When, however, the total average score of sessions involving alternate reward and no-reward runs has been compared with sessions in which no reward at all was given, the former average was much larger (**4, 6**). This finding has been called the "spread-of-incentive effect" and suggests that the offer of a reward in one part of an experimental session enhances the scoring rate of all of the trials made in that period. On the basis of the present PK experiment, the effect of the rewards cannot be conclusively determined since we do not have a control series in which no rewards were offered on any trials; it is conceivable, though, that the scoring rate in the present series was somewhat enhanced by the promise of rewards on half of the trials. However, it must be kept in mind that for some adult subjects the offering of a reward may be distracting and even somewhat embarrassing, especially since it may imply that the subject needs to have such ulterior motivation to arouse his interest in participating in a scientific test. Thus, the use of rewards needs to be carefully adjusted to the interest level and attitude of the individual as well as the group.

Position Effects

There was also, in this experiment, a general reversal of the direction of position effects usually found in the PK experiments. Instead of a preponderant decline, it appears that the distributions are all inclines (except for a drop in scoring rate from the first to the second session). First, there is a very steep incline in the chron-

ological pattern. There is also the rise in scoring from the first set to the second; and, of course, the diagonal distributions from the first to the fourth quarter of the QD's of the total page and the total set data showed inclines instead of the generally expected declines.

The Experimenter As the Real Subject

As an explanation of the unexpected results from the reward comparison and from the position effects, the hypothesis that seems most plausible is that the experimenter, A.R., is herself most responsible for the results. In the first place, the significant chronological incline is undoubtedly independent of the individual subject. While it is possible that the subjects of the first half were naturally low-scoring subjects and those of the second half were naturally high-scoring, it is most plausible to suppose that the subjects were not essentially different and that the experimenter herself underwent a progressive change, acquiring greater familiarity and deeper interest as the experiment advanced. If this is so, the other inclines would be likely to be associated with, and to some extent patterned after, the chronological incline—as they seem to be.

There is no way of reliably ascertaining which of the two persons present in the test is actually responsible for the PK that was exerted. It is logically plausible, however, to suppose that, of the two, the experimenter was the more strongly motivated toward discovering whether PK was equally possible at 30 feet and at three. If so, she would naturally exert stronger volitional action at the longer distance than at the shorter; whereas, generally speaking, the neutral and disinterested attitude of the subject would not lead him to favor either distance. We therefore submit the purely speculative suggestion that the results and the distribution are due to A.R.'s own PK effect.

On the hypothesis just stated it is more easy to understand the failure of the drawings to discriminate in the performance of the subjects in the PK test. In this respect they do not fit in with the only other data available on the question (1). The variations in scoring from one subject to another, if the hypothesis holds, would have been due to the fluctuations of the experimenter herself, and not to the subjects. Likewise, the peculiar effect of the use of re-

wards would be understandable if the subjects themselves were not responsible for the scoring rates.

It will require a still further experiment to separate the potential influence of subject and experimenter. The present report must therefore leave the question open, but with the findings favoring the hypothesis of the role of the experimenter as the effective subject. Progress may be claimed, however, on the question of the effect of distance.

References

1. Ellis, L. R., and Humphrey, B. M. Comparison of ESP and PK tests. [Unpublished research.]
2. Humphrey, B. M. Success in ESP as related to form of response drawings: I. Clairvoyance experiments. *J. Parapsychol.*, 1946, **10**, 78-106.
3. Nash, C. B. Position effects in PK tests with twenty-four dice. *J. Parapsychol.*, 1946, **10**, 51-57.
4. Rhine, J. B. Experiments bearing on the precognition hypothesis. III. Mechanically selected cards. *J. Parapsychol.*, 1941, **5**, 1-58.
5. Stuart, C. E. An ESP test with drawings. *J. Parapsychol.*, 1942, **6**, 20-43.
6. Woodruff, J. L., and George, R. W. Experiments in extrasensory perception. *J. Parapsychol.*, 1937, **1**, 18-31.

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IMPATIENCE WITH SCIENTIFIC METHOD IN PARAPSYCHOLOGY

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That the greatest advances in reliable knowledge have come about through the use of scientific method everyone would readily agree. There is an almost universal respect for the findings of scientific inquiry, not only in the physical sciences and in those of biology, but also wherever science has been able to penetrate into the province of human nature.

In the field of parapsychology, however, the increasing use of scientific method has not met with universal satisfaction. While, on the one hand, our experimental advances have succeeded in meeting the most stringent criticisms coming from outside the field, a growing volume of impatience has arisen from within, decrying the prospect of a wholesale conquest of parapsychology by means of the controlled quantitative experiment.

This disagreement, broadly speaking, is of two kinds: The first comes from those who believe that certain problems in parapsychology are beyond the reach of scientific method. These people are not ready to grant that rigorous experimental investigation is adequate to deal with all of the realm of nature that they claim for parapsychology. They hold that something unique must be conceded as deriving from subjective personal experience, something that only the mystic can reach by a necessarily individual contact with some transcendent order of reality, something perhaps like ESP itself utilized as a method of arriving at truth.

There are, of course, many in the world today who still hold that certain questions can be answered only by faith and philosophical speculation, but we are not now concerned with this group. No; those who are holding out for the truth-revealing efficacy of individual experience maintain, rather, that this experience offers another evidential contribution to parapsychology—something that scientific method cannot reveal—yet a contribution that is dependent neither upon mere faith nor upon speculation.

The second general type of criticism of scientific method in parapsychology is both more often and more strongly expressed. Those who offer it claim that while we have to be scientific we need not construe all science as necessarily quantitative, statistical, or even experimental. They urge that there is a broader and more adaptable conception of scientific method than that which is in current use in the laboratories where research in parapsychology is conducted. The present approaches, they say, are too narrow and, accordingly, are missing something important which freer methods would discover.

While these doubts and differences are usually expressed in friendly terms, it appears to many of us that such impatience with scientific method in parapsychology will have disastrous results if the disagreements are not clarified and resolved. For one thing, we cannot afford internal conflict in a group so small as the working personnel of parapsychology is today. A loss of confidence in our competence, too, will develop, both within and without the field of research, if we show serious division over our fundamental methodology. It is almost inevitable that such expressions of disagreement will underestimate the value of the work of those on the other side and that the human tendency for partisanship will accentuate the initial division until collaboration becomes impossible.

Such a situation, however, has not yet actually developed, though it threatens to do so. Fortunately the differences in attitude toward scientific method in parapsychology may, I think, be harmonized to the satisfaction of all. In fact, whatever our present position, we need to yield nothing of what we really value while coming to agreement with the other views, and we can achieve this accord entirely on the basis of a common understanding and acceptance of scientific method. There will doubtless still be differences of emphasis and interpretation, but they will not be on fundamental methodology and hence will not impede our rate and range of progress. An attempt at finding a common ground for accord on the basic strategy of scientific method in parapsychology is here submitted for consideration and discussion in the hope that it will offer a step toward greater harmony and better cooperation in the field.

* * *

The question behind the first type of criticism of scientific method in parapsychology is as follows: *Is there any kind of parapsychological experience necessarily beyond the reach of scientific method?* The answer will depend upon what we think scientific method is. It is my suggestion that this criticism arises out of a much too specialized and limited conception of scientific method, one that is not sufficiently flexible, one that is not correct. General scientific method is a very broadly adaptable plan of attack in solving a problem. But unless we are especially careful, we tend to identify it with the particular branch of science with which we are most familiar. Properly regarded it is not limited at all to any particular branch of scientific study or portion of the universe. Rather, it is logically applicable to any real problem anywhere. If science could not deal with personal experiences, we should have no scientific psychology. Any personal experience, no matter what kind of mental activity it involves, represents something actually going on in the universe. Anything that produces effects of any kind thereby gives us something to observe and study. These observations allow a foothold for scientific method.

It may, of course, be very hard for us at present to deal with the particular experience concerned. Special techniques will probably have to be developed to cope with it and they may be hard to devise; but no factors or agencies in nature can be assumed to be beyond the reach of science unless we also assume that they have no effect that is traceable, either directly or indirectly. If there is no effect we do not even know there is anything real involved. Obviously, then, there is no problem. If there is a trace of something, that is the place to start the inquiry.

Let us take as an example a typical mystic experience such as those familiar in religious literature. It might be either a dream or a waking vision. The mystic may report that he is transported to an extraterrestrial realm and there confers with spirit beings of one type or another. The special character of the problem raised will largely depend on the supposed identity of these spiritual beings and on the subject of the conference. For the sake of simplicity, let us content ourselves here with but a single one of several problems;

it will serve as well as any to illustrate in a general way how scientific method would be applied to the whole range of personal experiences. We shall ask: "Do such personalities as those with whom the mystic believes he communes have some existence independent of his experience? In a word, are they real?"

The *first* step is to make as clear as possible just how the problem should be stated. This step, like all the others, might be elaborated at great length, but in the space available I can barely outline the steps through which the method would take us. We may agree on the question: What is the real character of the purported beings described by the mystic? The *second* step would then be to collect all the facts from his report, to draw from him anything further that introspection and psychological analysis could reveal. This factual survey should take us into the study of similar visions and all such studies of them as have been made.

The *third* step would follow when the facts had all been collected and studied. We would list the whole range of trial hypotheses, or possible answers to the question, that we could assemble, even those which we held in disfavor. We cannot rely on our judgment at this point to know the true answer or we should not need to go on the rest of the way. Without attempting to be complete, I submit that we should consider the following hypotheses: (a) that the mystic is a fraud and that he deliberately or under delusion invented his entire story, utilizing knowledge normally acquired; (b) that he is sincere, and has had a dissociative experience in the course of which he made use of extrasensory perception to endow his vision with a transcendent order of knowledge; or (c) that he is genuinely in effective contact with incorporeal (or spirit) personalities having independent reality.

The *fourth* step is the rational weighing of these different hypotheses against the factual background (of step two). The decision is made as to which one offers the best logical case and therefore seems like the best prospect for further study. We may suppose for the moment that it turns out to be the last mentioned, the spirit hypothesis.

In that case the *fifth* step is relatively clear. It would consist of

a rational induction running something like this: *If* the mystic did have some kind of personal contact with real incorporeal personalities, *then* in the very nature of the case some sort of telepathy between incorporeal and corporeal personalities must have occurred. It ought to be possible, then, by some way and means to evoke and record further examples of such thought transference with the deceased in as far as we can approximate the conditions of the case under study. We need not assume that such conditions can be perfectly duplicated nor that repetition must inevitably follow if we did. The inference is that approximate duplication sufficiently increases the likelihood of recurrence to make the next step appear profitable.

The *sixth* step, then, is the designing of a proper experiment (or a series of experiments) which would distinguish between the working hypothesis (telepathy with spirit personalities) on the one hand and all the counterhypotheses on the other. Naturally we cannot expect that this would be an easy accomplishment. Indeed, I am not confident, as yet, how it is to be accomplished. Until after it is done and the experiment has been successfully carried out no one knows how difficult or how successful the undertaking will be. But it must be equally clear that we cannot say in advance that such an exploration is doomed to failure of completion, that the problem lies forever beyond scientific method.

There are several further steps involved that we can only mention for the present: (seventh) the proper conduct of the experiment as designed, (eighth) the appraisal and interpretation of the experimental results, (ninth) the correct phrasing of a conclusion, and finally, (tenth) independent repetition of the experiment. We need not elaborate on these, however, to make it clear that if we are ever to come to any reliable conclusions about the question raised, we will have to push on through these further steps of scientific method to do it. The individual mystic experience may initiate an inquiry, but later steps of verification are required to determine what interpretation is safe to accept.

The illustration just given indicates the part which an inexplicable personal experience would play in a scientific investigation. Many

inquirers, as we know, will be too easily satisfied at the fourth step, which is the arrival at a mere working hypothesis. Even in trying to answer the major questions that concern human life that is as far as the great majority of people care to go. They have not the time and patience, or they do not know how, to put their chosen explanation of things to a full and crucial scientific test. The expression, then, that "there are some things beyond science" is likely to find a too-easy acceptance with such people. As long as we think a problem is beyond the range of scientific method we simply do not try to solve it.

Those with whose criticism we are here concerned may still have some doubt as to whether there are not certain rare and delicate experiences that will defy all experimental effort to verify them. We cannot answer with certainty either way. We do know that the more rare and delicate a phenomenon is the more we will have to learn about it before we can reproduce and study it at will. In fact, the very elusiveness of a phenomenon is an indication of the inaccessibility to control of its governing principles. We therefore expect delay and difficulty, yes, but eventually a reliable explanation.

* * *

The second main line of criticism of scientific method in parapsychology is that such method is at present too narrow and artificial to be suitable for all the existing problems. The attack in this case is directed against the quantitative procedures, mostly statistical, and against the strict standards of experimentation that have developed. The general question before us, accordingly, is *whether or not scientific method can ever properly operate without these experimental and mathematical controls.* What this comes to in parapsychology is the question whether we can sometimes rely solely on the logical analysis of accounts of spontaneous experiences, or on reports of uncontrolled or semicontrolled observations on special individuals.

The issue, I think, is a simple one: It is a matter of how careful we wish to be in excluding alternative explanations before drawing conclusions. The mathematical and the experimental controls are merely techniques of safeguarding the inquiry against spurious fac-

tors that might lead to erroneous interpretation. The history of parapsychology is the story of progress achieved through these very improvements of method now questioned. It was only through the building-up of essential safeguards that the ESP experiments, for example, finally compelled serious attention from the scientific world. Anyone, then, who can see parapsychology in perspective can only view with amazement the suggestion that we discard these well-tried precautions.

In some instances the more elaborate controls might well be lightened somewhat for the more free-ranging and introductory explorations. On the other hand, so long as we are investigating extrasensory perception, for example, it would be folly to fail to maintain conditions that exclude the *possibility* of sensory perception. Likewise, so long as there is any possibility that chance can account for any of the data under investigation, we must *always* have methods for estimating probabilities. So long as we are comparing one set of test conditions with another, we must be ready with suitable mathematics to make the comparison reliable. If we aspire to anything beyond a mere loose impression we have no choice but to use the measurements required for accurate judgment.

This is not the place to review the actual progress in discovery that has already been made by means of controlled procedures. (These advances are to some extent reviewed in my recent book, as well as in other books that have come out during the last ten years.) But as a matter of fact, I would find it very difficult to point with confidence to anything that has been reliably established in parapsychology which has *not* first been put through the mill of quantitatively controlled experimentation.

What, then, do critics of present methods in parapsychology expect us to do? What other methods and approaches are there? In general, their emphasis is directed toward the study of spontaneous psi experiences, toward the discovery of either the rare outstanding phenomenon or the rare individual whose performance is so striking as not to need statistical treatment for its evaluation, or toward the analysis of mediumistic utterances so strikingly

appropriate as (in their judgment) to transcend the limits of all counterhypotheses.

In each such case we do have, I grant, something on which to *start* a proper scientific inquiry; the question then arises as to how to follow through. To illustrate, let us take a spontaneous psi experience, say a case of apparent telepathy. It makes no difference whether it crops up in the course of a psychiatric study of a patient, in an anthropological study, in an autobiography, or elsewhere.

The following (unverified) story, told in brief, is merely the latest one received at the time these lines were written: One of my students, a young woman, was working in an office last summer in a Southern town. Her father was employed in the city of Washington and was not expected home for some time. One morning (Thursday) the girl was puzzled to find herself unaccountably expecting him home over the weekend. When she went home at lunch time she found her mother making preparations for her father's return; she too had inexplicably got the impression at about the same time as the daughter. They both decided, however, that it was so unlikely that the father would come that they dismissed the matter from their minds. It turned out, they found out later, that at approximately the same time the father *had* suddenly decided (equally unexplainably) that he wanted to come home over the weekend and had asked his employer for permission. He could not get off, however, and he too dismissed the matter. There was an interesting sequel in the fact that on the following Sunday morning the father's father, who lived in the Southern town, died suddenly and the father did, after all, come home that weekend.

We can begin with whichever of several possible questions we want to single out; and, of course, the question we ask will determine the nature of what follows. But in order to make the example more clear let us begin with the very simple and elementary question, "What made the mother and daughter think the father was coming home?" That represents the *first* step in a scientific inquiry. The *second* would be to survey all the relevant facts that are obtainable from the case and from related material. The *third* step would be to list all the possible explanations, likely and unlikely: chance coin-

cidence; some common but unrecognized clue or reminder, such as a remark on the radio or something they both read in the paper or the like; the possibility of clairvoyance, since the mother and daughter may have been in contact with the father's action, his voice, or something he wrote rather than his thought; or the hypothesis that father, mother, and daughter were all unconsciously anticipating the death of the grandfather, either on the basis of unrecognized cues or by precognitive ESP; and finally the hypothesis of telepathy between the three.

Let us suppose that after careful rational analysis, as our *fourth* step we select telepathy as the most likely hypothesis. Who will say that this is the right place to *stop* the inquiry? Yet that is exactly the point at which many people want to discontinue the search in such a case as this—they would be content merely to multiply such case studies, compare results, and draw their conclusions on that basis alone. But to stop research at this pre-experimental stage is simply to discard all the verificational benefits of scientific method that have eliminated error and made science dependable.

Rather, a thorough-going science requires us to go on to a *fifth* step and derive from the working hypothesis (telepathy) a practical experimental test. To arrive at a verifiable induction, we reason: If telepathy be the true explanation, then we may expect to obtain a recurrence of telepathy if we can approximate the essential conditions of the case reported (and those of like cases).

The *sixth* step, then, is to design the kind of experiment that will not only approximate the kind of sender-receiver rapport that would favor the occurrence of telepathy but will also at the same time exclude all the possible alternatives known. Again, the other four steps, already described in brief, must be carried out too for the satisfaction of all those who are determined to be sure of their answers.

We know only too well that much philosophical bias, much wishful thinking, and general impatience come in to make it easy to stop somewhere along this arduous pathway. On the problem of telepathy especially there seems to be little interest in going beyond the working hypothesis stage, especially to the point of exclud-

ing the counterhypotheses of clairvoyance and precognition. The philosophical acceptability of telepathy is as strong for many of those interested in parapsychology as its unacceptability is for the orthodox psychologist.

There is nothing peculiar in the example used. It need not have had to do with telepathy. We could as well have started out with an illustrative case of so-called "psychometry" (clairvoyant object-association), a case of hypnotically induced "traveling clairvoyance," a veridical apparition, a witch doctor incident involving a prediction, an example of telepathic relationship between a psychiatrist and his patient, or even a report of a case of dowsing or of a poltergeist. Whatever it is, we are challenged to explain the reported phenomenon and we ask such questions about it as we are able or are interested in. Each question that spurs us enough may lead us on to step *two* and on still further until we reach a final answer; that is, if we care enough about the issue and the quality of the conclusion we accept to carry on beyond the point of philosophical satisfaction.

The objection has already been made that the experiment may hamper the natural expression of the phenomenon being tested. To some extent it will; but that is the nature of an experiment! It is, in fact, the very reason an experiment is useful. It holds back or controls part of the natural situation to which the phenomenon studied belongs; it eliminates certain of the variables and thereby enables something reliable to be learned about the others. Other kinds of experiments are needed for dealing with the factors eliminated. An experiment is obviously a one-sided, artificial approach to nature, something like photography. But by resorting to a series of increasingly better adapted experiments we come closer and closer to the obtainable facts about a phenomenon, as they are brought into focus. The creative imagination of the scientist does the rest in the synthesis of the results into a constructive whole.

Recognizedly, then, no experiment can fully reproduce the natural conditions under which a spontaneous psi phenomenon is reported to occur; are we not therefore likely to miss important truth by insisting upon experimental confirmation? Not at all, if we avoid

improper conclusions. If in step *six* we devise an experiment to test our working hypothesis and get negative results, we cannot conclude simply that the hypothesis was wrong. We have to be sure first that we provided a sufficient approximation of the conditions for it to operate, and certainty on that point is difficult. We may (and usually do) have to complete a *series* of experiments before we can allow a negative conclusion to be drawn. We may even have to go back to step *four* again and select a different working hypothesis and, of course, completely redesign our experiment to make it crucial.

The fear has also been expressed that parapsychologists may become so infatuated with the test methods that have brought success that they may make ends out of means and cut the problems to fit the techniques. We recognize that there is always likely to be an upsurge of activity following the introduction of every new methodological device that proves useful. This effect, however, merely indicates the poverty of techniques available. The remedy lies, not in disapproval of the techniques in use, but in the bringing in of new problems to be solved and in the design of new experimental methods to handle them.

* * *

I recognize, however—along with those who are impatient with elaborate procedures of verification—the very great importance of the nonexperimental observations and experiences in parapsychology. These occurrences are the well-springs of inspiration that refresh and extend the research interest of the inquirer and give us something to verify. It is vitally important to any branch of inquiry to keep in closest touch with the source material from which its problems arise. It would be extremely hazardous, even ridiculous, for us to assume that we have gleaned from the spontaneous happenings and personal observations of parapsychological nature all of the research clues and suggestions that they are capable of yielding. Far too little is known about the principles underlying the few psi phenomena that have been systematically studied to allow us to suppose that we have any comprehensive understanding of the fundamental nature of the mind. Indeed, some of our major questions regarding the nature of psi phenomena lead us almost directly back to the

study of spontaneous happenings, and here in the Parapsychology Laboratory such studies are actively under way.

Yet whatever fresh suggestions and working hypotheses we are able to extract from analyzing reports of uncontrolled observations of psi experiences, we shall not attempt to draw any final conclusions from any study based upon such material alone. We shall attempt at most to get from it some new hypothetical insights which can be subjected to crucial experiments for purposes of verification.

We must from now on, I think, stress with increasing vigor the importance of a wider exploratory front in parapsychology and a more systematic effort to get out of our raw materials some more penetrating trial hypotheses than we now have. But I would add at the same time that we need not, and dare not, relax any of the essential safeguards of verification that have put a sound foundation under the psi researches of the past. If we were to sanction any drawing of conclusions without the reliable methods of verification that have made scientific method what it is, we would only be undergoing a fatal regression to the childhood of our science. We would be wantonly surrendering our hard-earned gains.

No one will ask us, however, to discard any precautionary measures once it is clear that they do not stand in the way of a full exploratory advance in parapsychology. Since these measures are employed only after the exploratory work has been done, and since they perform the essential service of screening the hypothetical claims which the advance guard has picked up, their place in the total scheme must afford relief and satisfaction, instead of uneasiness, to every sincere inquirer.

Each worker in parapsychology, then, may find the task of his liking. Those who are more eager to range freely in the unexplored regions ahead of us may do us all a great service by bringing new problems to the laboratory—though the greater risks of these regions should be thoroughly appreciated. Those who are equipped with patience and training to do the designing of experiments and the devising of techniques, experimental and mathematical, have their own definite role to play, and a tremendous job awaiting them. There is even another specialized area concerned with the actual conduct of

the experiments. The personal relation of experimenter to the subject in attempting to educe the delicate phenomena with which we are supposedly dealing calls for a certain type of personality and personal interest even more limited, perhaps, than do the other functions.

No one can say which is the more important of these areas of interest, nor can any one of them be neglected. The fact that stands out most clearly at this point is that parapsychology has already become a complex field, requiring a broadly cooperative approach in the investigation of its problems. This very complexity places greater importance on its use of the guiding pattern of problem-solving procedures known as scientific method, all the way and in every way possible. The more thoroughly and understandingly we use it, the greater our total research progress and our general group harmony and satisfaction as well.

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THE RELATION OF ATTITUDE TO SUCCESS IN ESP SCORING

By J. M. Bevan

ABSTRACT: That the attitude of a subject toward ESP is important to success in the tests of the ability has already been reported by Dr. G. R. Schmeidler. Now Mr. Bevan goes further and shows how a group of 10 subjects who accept ESP compare with 10 who reject it, and with 10 who are undecided in their attitude. The student subjects were asked in personal interviews whether they believed ESP to be an established fact. They were then tested individually, both with general ESP and with clairvoyance techniques, mainly using card tests, but with a supplementary session with drawings as well. The ESP tests with drawings gave insignificant results, but the 352 runs through the ESP card deck, by the total of 30 subjects, yielded 112 hits more than the expected chance average. This result is significant, since the likelihood that it would occur by chance is approximately one in 1,000. The 10 undecided subjects were the highest; the 10 who believed in ESP were next; while those who rejected ESP were lowest. The last mentioned gave a score close to that expected from chance alone. The believers were most successful at clairvoyance, while the undecided subjects and the disbelievers did much better at general ESP.—Ed.

Introduction

One of the problems of the experimenter in parapsychology is to determine whether or not there are certain personality characteristics, which are found in high-scoring ESP subjects. Dr. Gertrude Schmeidler, looking to the attitudes of her subjects for some clues, thought it possible that those who believed in ESP might score higher than those who did not. She therefore classified her subjects according to whether they were "sheep" or "goats" (**4**). The sheep included those who thought there was some possibility of their guessing the ESP symbols to a better-than-chance degree and also those who were not sure. The goats were the ones who were convinced that any relationship between the target cards and their own guesses was purely coincidental. Dr. Schmeidler's belief that there might be a difference in the scoring of the two categories was borne out in a series of tests in which those grouped as sheep scored consistently above mean chance expectation while those grouped as goats scored consistently below (**5, 6**).

The experiment to be reported here has the same general experimental objective and procedure, with a few basic modifications. Dr. Schmeidler's subjects were divided into two classifications only: sheep and goats. The sheep, as explained above, included both those who accepted the possibility of ESP and those who were undecided. It seemed possible that those subjects who were "on the fence" might not be motivated as strongly by their attitudes as those who had an emphatic belief, either for or against ESP. Since the purpose of the research was to learn more about how attitude affects scores, it appeared that a distinction should be made between the two groups included by Dr. Schmeidler in her sheep classification. I decided, therefore, in planning the present experiment to divide the subjects into three categories: "sheep," "goats," and "indecisives."

A second feature of the experiment pertains to the targets used. In part of Dr. Schmeidler's experiments the targets were prepared decks of ESP cards which were located in a nearby office, and in part of the experiments they were merely folded sheets of paper bearing random lists of symbols. An attempt has been made, in this new experiment, to present the subject with a more simple and clearly defined target situation. The subject's efforts were always directed to a particular deck of cards; and at the time each call was being made, the specific card at which the subject was aiming was on the top of the pile, and its location was known to the subject.

The Experiment

Selection of Subjects

The experiment was begun at Duke University on March 1, 1946, and was completed on August 6, 1947. The completion date was delayed because of the increasing difficulty of finding the required number of goats who were willing to cooperate. I myself participated as the experimenter in all the experimental sessions, using as subjects 30 university students (11 women and 19 men) who were selected on the basis of their attitudes toward the possibility of ESP.

Each prospective subject was interviewed before he was selected, and during the interview, which sometimes lasted as long as an hour, he was asked the following question: Do you accept ESP as

an established fact? Those few students who gave an emphatically negative answer to the question usually argued by way of explanation that ESP would not fit in with their physicalistic philosophies or else they merely asserted that ESP was fantastic, irrational, and wholly inconceivable. A few even said, "If it were so, I wouldn't believe it." All of these, of course, were classified as goats.

Most of the men and women of whom the question was asked were not sure; they expressed the view that maybe ESP occurs, and maybe it does not, and that whichever is true, it was of no immediate personal importance to them. Others admitted their lack of knowledge of the field of parapsychology and felt as if they were in no position to offer an opinion. All these were classified as indecisives.

Those who accepted ESP as an established fact gave, in general, one of two reasons for their belief. First, they had had, at some time in their lives, some psychical experience; or, second, they had become acquainted with experimental findings which had convinced them.

In the course of the interview several of the experimental laboratory methods of measuring ESP were demonstrated and explained to the students. In the case of those who said they considered ESP to be possible, a second question was asked: Do you think ESP can be measured by the techniques just explained to you? Those who answered "Yes" were chosen as sheep; those who said "No" or "I don't know" were disqualified as subjects.

As an additional check on attitudes, all of the subjects who were chosen were asked to fill out a questionnaire at the beginning of the first test session. In it were repeated the questions already mentioned. On the paper a straight line was drawn to represent the continuum from belief to disbelief in the possibility of ESP. Half way between the extremes was a question mark to represent indecisiveness. The student was asked to place a check on this line at the point which would represent the extent of his belief or disbelief in the possibility of ESP. This attitude questionnaire confirmed the original classification of the subject in every case. The subjects were also asked how well they thought they might do in the tests to follow and how they felt physically. In one case a subject was

emotionally upset and was given an appointment to come at another time.

Of the 30 subjects chosen, 10 were sheep, 10 were indecisives, and 10 were goats.

Procedure

Care was taken in this experiment to guard against the altering of the social atmosphere of the experimental situation in favor of any one group. All persons were tested in the same room under the same objective conditions. Furthermore, the experimenter was careful not to speak words of encouragement or discouragement to any of the subjects. No special privileges were granted to any group and no special incentives were offered.

In the first experimental session, the subject was taken into the room in which he was to engage in the tests. At this time the ESP symbols were shown to him; he became acquainted with the method of recording; and he was instructed in the use of the signaling apparatus. He was then given the questionnaire already described, and when he had repeated the ESP symbols over again to refresh his memory, the card runs themselves were started.

Each subject participated in three sessions of four runs each, making a total of 12 runs per subject.[1] A session comprised two GESP and two clairvoyance runs, taken alternately one by one, with a GESP run followed by a clairvoyance run, or vice versa. In most cases the first session started with GESP; but whatever the order, it was reversed in the second session. The third session duplicated the first. Successive sessions for the same subject were never held within 10 hours of each other.

The method of testing used in the clairvoyance part of the experiment was the distance BT technique with ESP cards in which the subject, in one room, was to try to identify the cards which were taken off the deck, one by one, by the experimenter in another room 40 feet away.

The experimenter first shuffled the deck thoroughly by hand and turned it face-down. After he had given the subject time enough

[1] One sheep and one indecisive failed to come for a third session. The sheep and indecisive groups therefore contain four runs less than the goat category.

to ready himself, he removed the top card from the deck, laid it, still face-down, on the table, and pressed the button of his signaling device. This turned on a light in his room and one in the subject's room. These lights could be turned off, however, only from the subject's room. When the subject saw his light come on, he made his first call, recorded it, and pressed his signal button, turning out both his own light and that in the experimenter's room. The experimenter then removed the second card from the deck, placed it on top of the first one, and pressed his signal button once more. The subject recorded his call for this trial and again turned off both lights. This process was repeated until all 25 choices had been made.

The same procedure was used in the GESP runs except that in the latter the experimenter looked at each card while the subject was trying to identify it by ESP.

Each set of four runs was recorded by both the subject and the experimenter on standard ESP record sheets with carbons. The subject recorded each call as he made it. The experimenter, however, recorded the target deck at the end of each run of 25 cards in the order in which he had gone through the deck. At the end of each run he took his record to the subject's room, checked the hits, and totaled the score. He then returned to his room and the second run was done. At the end of each experimental session, the carbons were turned over to Dr. Betty M. Humphrey, record librarian, the experimenter retaining the originals from which to make his analyses.

As an incidental part of the main experiment, a series of ESP tests based on drawings was introduced. These tests grew out of the work of Dr. Humphrey on the discrimination of high and low scoring in ESP on the basis of the form quality of ESP response drawings (**1, 3**). Dr. Humphrey reported that the subjects making the more expansive drawings in the clairvoyance tests gave higher scoring rates than those making compressive drawings. The opposite relation was found in the drawings made in GESP tests. The purpose of introducing these drawings into the sheep-goat-indecisive research was to discover the relationship, if any, of expansiveness and compressiveness to each of the three attitude categories. Each subject was therefore asked to come for a fourth session during

which he made four ESP drawings, two with the clairvoyance technique and two with GESP.[2] If the first drawing was a clairvoyance drawing, it was followed by a GESP drawing; the third was the same as the first and the fourth was the same as the second. If the first was a GESP drawing, the order varied accordingly.

The pictures used as stimuli were taken from a large collection cut from magazines and mounted on cardboard. They were selected by Dr. Humphrey in units of four and were enclosed by her in opaque manila envelopes. For the clairvoyance part of the experiment the envelope was sealed and neither the subject nor the experimenter, of course, knew what the picture was. The subject did this part of the experiment in the same room in which he had taken the card tests. He was given one of the envelopes which contained a picture and which had a sheet of paper fastened to the outside. He was asked to draw, without looking inside, his response to the picture within. The experimenter remained in his own room allowing about five minutes for the subject to make his drawing, whereupon he entered the subject's room and took the envelope and drawing.

In the GESP part of the experiment the envelope containing the drawing was not sealed. The experimenter, stationed in his room, removed the picture from the envelope and looked at it for about five minutes while the subject was doing his drawing in the other room. When the four drawings were finished, the experimenter coded the stimulus pictures and responses for future checking. After the entire experiment was completed he turned over all the stimulus pictures and response drawings to Dr. Humphrey, retaining the code himself.

The drawings were first rated for expansion-compression by Dr. Humphrey according to the method described in a previous report (1). Then in order to obtain an ESP score for this material, two independent judges, Dr. Humphrey and Dr. C. E. Stuart, matched the drawings and targets[3] by the Stuart preferential match-

[2] The decision to add this part of the experiment was made after the card tests had been completed. By that time, three subjects had left college and were not available for additional work. Hence there were only 27 sets of drawings.

[3] After Dr. Stuart's death, the remainder of his share was completed by Miss Elizabeth A. McMahan.

ing method (7). The card data, the drawing data, and the statistical analyses were all independently rechecked under Dr. Humphrey's supervision.

Results

Card Runs

The total of 352 card runs yielded 1,872 hits where chance expectation is 1,760. This deviation of 112 hits gives a significant CR of 2.99, with a probability of .001. (See Table 1.) An insignificant deviation of +42 was obtained in the 176 runs using the clairvoyance procedure; and in the same number of runs in the GESP tests, a deviation of +70 occurred, giving a CR of 2.64, with a probability of .004.

Table 1

ESP Scores on Cards for Clairvoyance and GESP Conditions

	Runs	Dev.	Av. Score	CR
Clairvoyance.....	176	+ 42	5.24	1.58
GESP.....	176	+ 70	5.40	2.64
Total.....	352	+112	5.32	2.99

Table 2 shows how these results were distributed among the different attitude groups. The sheep obtained higher results in the clairvoyance runs than in the GESP. Their 58 runs in clairvoyance gave a deviation of +39 with a CR of 2.56; whereas their work in GESP gave a deviation of +5, with a CR of .33. The total of all clairvoyance and GESP runs for the sheep gave a deviation of +44 with an insignificant CR of 2.04.

The indecisives, besides scoring highest of the three groups, did better in the GESP situation than in the clairvoyance. The 58 GESP runs gave a positive deviation of 52 with a CR of 3.41, and their clairvoyance work gave a positive deviation of 14 with a CR of .92. The total of 116 runs in both GESP and clairvoyance tests gave a positive deviation of 66 with a CR of 3.06. The combined total work of the sheep and indecisives yielded a positive deviation of 110 with a CR of 3.61 ($P = .0001$).

Table 2

ESP Card Scores as Related to Attitude Groups

	Sheep				Indecisives				Goats			
	Runs	Dev.	Av. Score	CR	Runs	Dev.	Av. Score	CR	Runs	Dev.	Av. Score	CR
Clairvoyance	58	+39	5.69	2.56	58	+14	5.24	.92	60	−11	4.82	.71
GESP.....	58	+ 5	5.08	.33	58	+52	5.89	3.41	60	+13	5.22	.84
Total...	116	+44	5.39	2.04	116	+66	5.57	3.06	120	+ 2	5.02	.09

The goats scored at chance. From the 60 clairvoyance runs a deviation of −11 was obtained and from the same number of GESP runs there was a positive deviation of 13. The total 120 runs had a deviation of +2, which is not significantly different from chance expectation.

When the total results are divided into separate scores for the three experimental sessions, definite scoring trends are revealed. The sheep, as shown below, definitely improved from session to session, while the indecisives just as consistently declined. The goats showed only a slight variation.

	Sheep		Indecisives		Goats	
	Runs	Dev.	Runs	Dev.	Runs	Dev.
Session 1	40	— 3	40	+32	40	+4
Session 2	40	+21	40	+24	40	+1
Session 3	36	+26	36	+10	40	—3
Total	116	+44	116	+66	120	+2

(For what interest the observation may have, it may be said that the sheep, who improved their scores from session to session, seemed to maintain their enthusiasm throughout the experiment. The indecisives, whose scores declined, on the other hand, seemed increasingly prone to express their dissatisfaction.)

It will be remembered that each session consisted of two runs of clairvoyance and two runs of GESP. The two runs made under each procedure were considered as a unit for study of position effects. This two-run unit was quartered; that is, each run was divided in half (the thirteenth trial in each being omitted), and the deviation

for each half-run was found. These quarter distributions (QD's) of the two-run "set" were pooled for all the subjects. The following QD was obtained:

84.48 +19.6	84.48 +43.60
84.48 +18.6	84.48 +20.60

There are no significant differences between quarters, but it is interesting to note that the general pattern shown here is like that of previously reported QD's for ESP test results (2); that is, the largest deviation occurs in the upper right quarter, while the smallest appears in the lower left quarter.

Drawings

There was a total of 27 sets of drawings, each set comprising two GESP and two clairvoyance drawings. Thus there were 27 GESP pairs and 27 clairvoyance pairs. The ESP scores on the drawings, which were obtained from the preferential matching, are presented for each attitude group in Table 3. They have a total deviation of +49 with a ("Student's") t of 1.27 (d.f. = 53).

Table 3

ESP Drawings Results as Related to Attitude Groups

	Clairvoyance				GESP				Total			
	No. of Pairs	Dev.	Dev. of Mean	t	No. of Pairs	Dev.	Dev. of Mean	t	No. of Pairs	Dev.	Dev. of Mean	t
Sheep.....	8	+7	+.88	.79	8	+18	+2.25	1.10	16	+25	+1.56	1.38
Indecis.....	9	+1	+.11	.05	9	+32	+3.56	2.80	18	+33	+1.83	1.40
Goats.....	10	−9	−.90	.44	10	0	.00	.00	20	− 9	− .45	.37
Total.....	27	−1	−.04	.04	27	+50	+1.85	1.99	54	+49	+ .91	1.27

Though none of the drawings results are statistically significant, the indecisives and the goats display a trend similar to the one evident in the card calling. The indecisives definitely seem to do better in GESP.

When the drawings were divided into expansive and compressive groups and compared with respect to their ESP scores, no statistically significant results emerged. Table 4 shows the results of this comparison for the sheep, indecisive, and goat groupings.

Table 4

ESP Drawings Results for the Expansive and Compressive Subjects in Each Attitude Group

	GESP									CLAIRVOYANCE								
	Expansive				Compressive				t_d	Expansive				Compressive				t_d
	No. of Pairs	Dev.	Dev. of Mean	t	No. of Pairs	Dev.	Dev. of Mean	t		No. of Pairs	Dev.	Dev. of Mean	t	No. of Pairs	Dev.	Dev. of Mean	t	
Sheep....	4	+11	+2.75	1.36	4	+ 7	+1.75	.46	.23	4	+ 7	+1.75	2.05	4	0	00	00	.77
Indecis...	4	+14	+3.50	2.94	5	+18	+3.60	1.62	.04	6	+16	+2.67	1.08	3	−15	−5.00	1.67	1.87
Goats....	6	− 4	− .67	.29	4	+ 4	+1.00	.52	.53	3	−13	−4.33	1.18	7	+ 4	+ .57	.24	1.14
Total..	14	+21	+1.50	1.25	13	+29	+2.23	1.50	.38	13	+10	+ .77	.50	14	−11	− .79	.53	.72

In the total GESP results the mean score of the compressives is higher than that of the expansives while in the total clairvoyance results the reverse relationship is true. Both of these trends, while not significant, are in agreement with previous studies of the relation of expansion-compression to GESP and clairvoyance test results. When the drawings results are considered separately for the different attitude groups, none of the differences between expansive and compressive subjects are significant.

Discussion

The CR of 2.99 for the total ESP card runs is significant. On the basis of this CR and others of extrachance nature in special groupings of the data, the hypothesis that chance is the explanation of the results may be ruled out. The experimental set-up adequately

prevented the possibility that the subjects could have made use of sensory cues to obtain their extrachance scores; and since the data were all independently rechecked, the hypothesis of checking errors does not stand up. It appears that ESP is the explanation of the results.

The purpose of the experiment was to determine whether there was a difference in scoring between subjects who accepted ESP as an established fact and those who did not or who were indifferent. The results have borne out the previous experimental findings of Dr. Schmeidler that sheep consistently scored higher than goats. The experiment shows, in addition, that the third class of subjects, the indecisives, who had been included with the sheep by Dr. Schmeidler, scored higher than either of the other two classes. The differences, however, were not significant.

These facts directly introduce the question: Why should the three groups have scored as they did? Relatively speaking, the goats maintained the level expected of them on the basis of Dr. Schmeidler's experiment; that is, they scored lower than the sheep. They did not, however, score consistently below expectation as has been the case in seven of the eight published Schmeidler series. This difference may be due to the difference in the target situation, and particularly to the use of the GESP procedure in the present experiment. All of Schmeidler's subjects were tested with a clairvoyance procedure; and in the clairvoyance tests of the present series, the goats did score below expectation.

The answer as to why the indecisives scored higher than the sheep, however, can at this stage be only conjectural, but there are certain observations which may be enlightening. The indecisives approached the experiment in a manner which at the start was favorable to good scoring in ESP. They had a free-and-easy attitude. Without any great stake in the outcome, they were motivated by no particular urge other than mild curiosity, and they could therefore be free of constraint and inhibition. This attitude is generally accepted as conducive to good scoring; but in the case of the indecisives, the initial curiosity was hard to maintain because of the general indifference of these subjects. It is possible, then, that

the high initial scoring was gradually offset by the boredom which was noted by the experimenter as the work progressed, and that the decline was the natural result of waning interest.

The sheep, by way of contrast, had emphatically declared themselves to be sure of the possibility of ESP, and most of them at their first interview had recounted, at some length, experiences of a personal nature which had convinced them of the occurrence of ESP and, in some cases, of their own ESP ability. A strong, conscious effort to succeed, while it may provide good motivation and sustained interest in the long run, may introduce accompanying activities that inhibit the unconscious ESP process. Moreover, the sheep were the only subjects among the group who had experienced ESP and who might consequently find that this new type of rigidly controlled experiment called for readjustment on their part.

It is possible, therefore, that in these facts lie the reasons, not only for the initial differences in scoring of the sheep and indecisives, but also their respective incline and decline in the subsequent sessions when the experiment was well under way and they had had the opportunity to become thoroughly accustomed to the situation.

It would have been interesting, if the project had required additional weeks of experimentation, to have observed whether the sheep would eventually have surpassed the indecisives in over-all average scores.

Only a suggestion can be given to account for the fact that the sheep did significantly high scoring in clairvoyance while the indecisives and goats did better at the GESP tests. GESP allows for the possible functioning of telepathy, and since the hypothesis of telepathy is more readily accepted by the average person than the hypothesis of clairvoyance, it is likely that the subjects preferred the GESP technique. Therefore the sheep, characteristically, may have tried hardest on these tests and, trying so hard, may have hindered their own scoring.

In this experiment, the ESP drawings tests gave insignificant results for all attitude groups. All three groups obtained higher scores in the GESP drawings tests than in the clairvoyance tests. Thus the experiments on the drawings show the same trend as those

on the card tests for the indecisives and goats, but not for the sheep (who did better in the clairvoyance card tests than in the GESP card tests).

The division of the drawings scores by the expansion-compression criterion revealed no differences of significance. The trend shown by the total results is in agreement with previous studies utilizing the expansion-compression measure; that is, the expansive subjects did better at clairvoyance than did the compressive subjects, while the reverse was true for the GESP tests. This trend, however, was not true of the separate results of the attitude groups in all cases (the two exceptions occurred in the GESP results of the sheep and in the clairvoyance results of the goats).

A desirable refinement of this experiment would provide for keeping the experimenter in ignorance of the attitudes of the subjects with whom he was experimenting. If they were classified independently, there would be no possibility that his knowledge would betray him unconsciously into treating one group differently, in a social sense, from another and thereby influencing their scores. For the same reason it would also reduce the chances of his varying the nature of his efforts as a sender. Until the actual influence of the sender is determined, such a control is to be recommended.

References

1. Humphrey, B. M. Success in ESP as related to form of response drawings: I. Clairvoyance experiments. *J. Parapsychol.*, 1946, **10**, 78-106.
2. ———. Further position effects in the Earlham College Series. *J. Parapsychol.*, 1945, **9**, 26-31.
3. ———. Success in ESP as related to form of response drawings: II. GESP experiments. *J. Parapsychol.*, 1946, **10**, 181-96.
4. Schmeidler, G. R. Predicting good and bad scores in a clairvoyance experiment: a preliminary report. *J. Amer. Soc. psych. Res.*, 1943, **37**, 103-10.
5. ———. Progress report on further sheep-goat series. *J. Amer. Soc. psych. Res.*, 1946, **40**, 34-35.

6. ———. Separating the sheep from the goats. *J. Amer. Soc. psych. Res.*, 1945, **39**, 47-49.
7. STUART, C. E. An ESP test with drawings. *J. Parapsychol.*, 1942, **6**, 20-43.

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BOOK REVIEWS

HANDBOOK OF TESTS IN PARAPSYCHOLOGY, by Betty M. Humphrey. Durham: Parapsychology Laboratory, Duke University, 1948. $2.50.

Experimental methods are the tools with which the scientist does his work. The development of any scientific field depends largely upon the variety and suitability of these tools. This is true to such an extent that a comparison of the methods in use at different times in the history of a branch of science is often a good representation of the amount and kind of progress made in the field.

By this standard, it is clear that Dr. Humphrey's handbook marks the end of a decade of significant progress in parapsychology. This volume is the successor to a little book entitled "A Handbook for Testing Extra-Sensory Perception" which appeared in 1937, and which is now out of print. A brief comparison of the scope of the two books will show clearly how much the field of parapsychology has grown during the intervening years.

The earlier book was devoted entirely to a description of card tests of the familiar aspects of ESP: clairvoyance, "telepathy" (as the hypothesis was understood at that time), and general extrasensory perception, or GESP. The present volume devotes not more than a quarter of its pages to card tests, in spite of the fact that these descriptions of the standard procedures in use today are far more refined and advanced than were those of 1937.

The earlier book was directed toward the interested amateur who wanted to satisfy his own curiosity regarding the occurrence of ESP in himself or among his friends. Whenever these pastime explorations yielded significant results, the authors encouraged the reader to carry on further tests under adequate scientific conditions, but it was left to the amateur explorer to take the initiative. Dr. Humphrey's approach is one which is more appropriate for the present status of the field. Her descriptions of the methods are aimed at the people who already have a genuine scientific interest in the subject and who start out with the intention of making a serious

contribution to knowledge. The ESP card tests in this volume are based upon actual laboratory procedures and are designed to eliminate such hypothetical factors as recording errors, sensory cues, checking mistakes, and all the other counterhypotheses to ESP.

The greater part of the book is devoted to the more significant developments of the decade. One of these is concerned with the problem of precognition or the relation between ESP and time. Dr. Humphrey describes the methods which have been developed and successfully applied in bringing this crucial problem into the experimental laboratory. An adequate procedure for testing precognition must, of course, eliminate all conceivable counterhypotheses, not only those which apply to the usual parapsychological experiment, but also those of ESP-of-the-present and of PK as well. For this reason, the precognition tests described are somewhat more complicated than most of the other methods included in the book, but they are not too difficult to be applied by anyone with a high degree of intelligence, patience, and good judgment. The investigator who is attracted to the problem of precognition because of the great scientific importance of the problem may, therefore, be able to make a lasting contribution to knowledge on this parapsychological front.

The problem of making an experimental test for pure telepathy in such a way that the results cannot be explained in terms of either clairvoyance or precognition is another one which has come into focus during the past decade. The method devised for this purpose is one of the most difficult of the standard procedures with which the parapsychologist deals, but Dr. Humphrey has given an excellent introduction to the experimental problem and to the basic principles of the test. She then refers the reader who wishes to become better acquainted with this procedure to the appropriate research reports.

Another ESP procedure which has become standard equipment of the research worker during the past decade is that of making tests based upon pictures. In these tests the stimuli are unlimited in variety, and the subject is told to make his response by drawing or writing anything he wishes in relation to the target picture. Tests based upon the correspondence between target pictures and subjects' drawings are not new, but results of this type have been put on a

new footing as scientific evidence by the development of an adequate objective method of evaluating the results. In the discussion of the drawings tests, Dr. Humphrey describes all that the experimenter who is new to the field needs to know to conduct experiments using this kind of material.

This handbook also describes the basic procedures now in use for testing psychokinesis, or PK. Thus it offers the prospective investigator a wide range of problems and procedures from which he may choose when planning his research.

Instructions for making preliminary evaluations of the results of each type of test are presented. The author makes excellent use of tables for the benefit of readers who lack experience with statistics. At the same time, she has explained the more basic evaluative methods in such clear, nontechnical language that the beginning experimenter will be able to measure his own findings even beyond the range of the tables.

Parapsychology is today in a phase of its development in which it depends heavily upon the research contributions made by intelligent amateur investigators. In this respect, it is like present-day astronomy, though the parallel with an earlier phase of astronomy is even closer. There are simply not enough people who are able to devote themselves to parapsychology as a full-time profession to accomplish more than a small fraction of the work that needs to be done. Under these circumstances, the professional worker stands to gain if he devotes a part of his time to the encouragement and supervision of newcomers to this research field. At the same time, such guidance costs an appreciable amount of the time and energy of those who are already experienced in doing research. It is, under these circumstances, simply a matter of wisdom to be as efficient as possible in making the necessary information available to those who will use it, and thus bring about a net gain to the research. This is precisely what Dr. Humphrey has attempted to do in this handbook of experimental methods, and her effort seems to have been highly successful.

J. G. PRATT

THE PERSONALITY OF MAN, by G. N. M. Tyrrell. Harmondsworth (England): Penquin Books, 1946. Pp. 295. 1s.

Readable books surveying what has been accomplished in parapsychology are especially needed today. A great accumulation of research findings has by this time been stored up in the technical periodicals. The general public and professional groups who might be interested know little about it. This collection of research material needs to be interpreted and presented in simple nontechnical language before it can be expected that many people will understand what has been achieved in this branch of science.

Knowledge of current developments reaches the public most extensively through the science columns of the daily press and weekly magazines. The short items that appear in these columns, while they have been very helpful and important in an educational way, cannot present enough of the background and framework of thought that goes with the research to give the general reader an adequate appreciation of its importance. Even the articles appearing in the monthly magazines have to be written on such a simplified level that they rarely contribute much to the general understanding of the larger issues of parapsychology. It takes a book to do the job.

It is, moreover, not just one particular kind of book that is needed; no single work can be expected to meet all the requirements. There are now many viewpoints and aspects represented in the field, and none of us would want to undertake to deal with them all in one volume. Our only safeguard against incomplete representation is to have many books and to have them written from different points of view.

Mr. Tyrrell's book is one that will help a great deal. It is easy to recommend it as good reading to everyone who is in any way interested in the field of parapsychology. It is very clear and understandable and is written from a viewpoint that needs to be represented. The author's long experience in research and writing in connection with parapsychology has qualified him to write with a broad perspective.

What viewpoint does he represent? What aspects and angles are outstanding in this volume? First of all it is an English book

and a book about English work, though there are brief treatments of some of the American and French studies. Fortunately, however, there has been enough research in parapsychology in England that if an author naturally centers his attention upon that territory he certainly has no lack of material. We who are less familiar with that locale and its research can only profit by such concentration. It is one of the amazing things in parapsychology that so much of its fruitful work has come out of the comparatively small area of Great Britain.

A second feature of Mr. Tyrrell's book is the fact that he gives major attention to the less strictly controlled experimental work, to spontaneous psi experiences and to semi-exploratory types of studies such as those of mediumship. As a result, he is able to bring before the reader a somewhat wider range of exploratory material than the more rigorous methods of inquiry have so far yielded. It is profitable to have these challenging problems freshly presented to us, even while they are awaiting the fully scientific treatment needed for their solution. Mr. Tyrrell himself has for some years been in full revolt against what he calls the "statistical experiment," or the use of quantitative methods in appraising the extrachance character of psi phenomena as a means of finding out about their nature. In his latest book he gives bare mention even to his own experimental work in ESP, although in his *Science and Psychical Phenomena* in 1938 he wrote at considerable length about it and about similar studies. But there are other books that review for the general public the research findings passed over by *The Personality of Man,* whereas the sector of parapsychology covered by Tyrrell's book has long been neglected, and the volume is therefore needed.

Still another of the outstanding characteristics of Tyrrell's book is its peculiar treatment of telepathy and clairvoyance, the two types of ESP. The fact is that, even though most of the ESP research reported has been of the clairvoyance type, none of this is presented (as clairvoyance) in the book. Even the word "clairvoyance" is only just mentioned, but the word "telepathy" is conspicuously present throughout and its importance is strongly accentuated. All of this comes from Mr. Tyrrell's philosophical position against clair-

voyance and in favor of telepathy. Once more, however, without in any way attempting to justify the author's speculative stand, we may grant that he still covers a large enough area of the field. The reader may, as it is, feel confident that the telepathy hypothesis has been worked to the limit of its possibilities in Mr. Tyrrell's treatment, and the student can always turn to other pages for the great volume of work now completed on the clairvoyance type of ESP.

Another feature of *The Personality of Man* is its major emphasis on the hypothesis of personal survival. Whatever one's attitude toward or interest in this question, he must recognize that a summary of the present status of the hypothesis is needed. A great deal more attention has been given to this hypothesis in England than in any other country; and the findings have even led a number of able English scientists and other scholars to the personal conviction that some kind of survival beyond bodily death does occur. Mr. Tyrrell, in his survey of the evidence, argues from a manifestly favorable viewpoint on the hypothesis, though he wisely does not attempt to present the case as a scientifically conclusive one. This treatment should help to sustain the interest needed for an exhaustive and well-rounded scientific approach to the problem.

The book is full of interesting personal and historical material. A section on attitudes toward the subject contains many items that everyone will enjoy reading. The final chapters on the significance of the whole study bring out the full maturity of Mr. Tyrrell's many years of thinking over the problems and the advances made in parapsychology. (He joined the Society for Psychical Research in 1908 and became its President in 1945.) The greatest value of the findings as the author sees them is their relation to religion. In his choice of the title of the book, Mr. Tyrrell indicates the fundamental significance he believes parapsychological phenomena have for the understanding of man as a whole. He indicates clearly that he is looking to parapsychology to enable religion to survive in the minds of thinking men.

J. B. RHINE

SPIRITISM, by G. H. Estabrooks. New York: E. P. Dutton & Co., Inc., 1947. Pp. 254. $3.00.

In this book, Dr. Estabrooks presents a broad survey of parapsychological phenomena from the point of view of the professional psychologist. Among the topics considered are spirit communications from trance mediums, telepathy, ghosts, "psychometry," precognitive dreams, mental healing, materializations, and other phenomena of physical mediumship. The author's purposes seem to have been two in number. First, he indicates the various normal explanations which may account for psychical phenomena. Second, he contends that recognized psychological explanations do not fully account for all the evidence of parapsychical occurrences. Thus the role of the unconscious mind in psychic experiences, the importance of suggestion and auto-suggestion in generating hallucinations and delusions, the parallel between trance personalities and well-known cases of multiple personalities, and the limitless possibilities of fraud are exhaustively considered in order to warn the naïve student from accepting the claims of psychical research in a nonscientific manner. Yet a residue of unexplained material is sufficient to convince Dr. Estabrooks of the reality of telepathy, apparitions, and even of survival.

To those engaged in parapsychological research there is nothing new in Dr. Estabrooks' imposing list of counterhypotheses, and there is much that is left out of it. For example, the degree to which chance factors may contribute to spontaneous and trance phenomena is disregarded, with the consequence that statistical controls against the chance hypothesis are not even mentioned. Also, parapsychical principles which might account for purported spirit communications are restricted to the telepathic and spiritistic hypotheses. Clairvoyance, despite the conclusive experimental evidence for its existence, receives no consideration.

For some curious reason Dr. Estabrooks can see little hope for laboratory methods of parapsychological investigation. He declares bluntly that "all laboratory attempts have so far failed to prove the reality of telepathy." Stating vaguely that the laboratory results of Coover, Warcollier, Rhine, and himself are only inconclusive, he

suggests that the best methods of proving the reality of supernormal occurrences are through the study of trance mediums, apparitions, and "psychometry."

Besides confusing telepathy with general extrasensory perception (his own so-called telepathy experiment was not at all specific for that capacity), Dr. Estabrooks fails to mention the large number of investigators who have achieved significant results through laboratory techniques. By omitting the contributions of such research workers as Soal and Goldney, Whately Carington, Tyrrell, Riess, Warner, Pratt, Stuart, Humphrey, Woodruff, Schmeidler, Martin and Stribic, Price, Pegram, and a great many others, the author reveals a surprising ignorance of experimental parapsychology. Incidentally, as Rhine points out in *The Reach of the Mind,* Dr. Estabrooks' own ESP experiment at Harvard is actually much more conclusive than the author ever supposed, even at the time of writing *Spiritism.*

The author's views on the survival problem are difficult to reconcile with those of a scientist. While Dr. Estabrooks is convinced of survival on the grounds of philosophy, religion, psychology, and psychical research, he believes the problem belongs properly to the philosopher or the theologian. Equally as surprising as this attitude is the author's failure to report many important contributions to the subject of spiritism in the literature of psychical research. Evidences from cross-correspondence and proxy sittings, as well as Whately Carington's quantitative study of trance personalities, constitute some of the more serious gaps in Dr. Estabrooks' account. It is true that the author is mainly preoccupied in this book with presenting the psychologist's typical explanation of psychical phenomena. Yet one regrets that he could not have combined with this objective a more adequate appraisal of both ESP research and the scientific studies on the problem of survival.

Because of the limited scientific interest in spiritism, this subject has largely been left to the inevitable distortions of the bereaved amateur, the professional medium, or the avowed spiritualist. Thus a book which deals adequately with the history of survival research and the present status of the spiritistic hypothesis is much needed

today. It is unfortunate that Dr. Estabrooks, a psychologist, should have undertaken to write such a book without sufficient preparatory study and experience.

For a more scholarly account of survival evidence than has been provided by Dr. Estabrooks, the reader's attention is directed to three articles by Dr. Gardner Murphy in the *Journal of the American Society for Psychical Research* for January, April, and October, 1945; and also to W. H. Salter's able review of these articles in the *Journal of the Society for Psychical Research,* London, for October-November, 1946.

WILLIAM R. BIRGE

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

*For a simple description of those terms marked by an asterisk, as they apply to the ESP test data, see Chapter VIII and the Appendix of *A Handbook for Testing Extra-Sensory Perception* by C. E. Stuart and J. G. Pratt. Further explanation may be found in any elementary statistical text.

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

CALL v.: To attempt to identify by ESP a stimulus object or a mental state of an agent.

CALL n.: The response described above; also the resulting selection.

*CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.
CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.
CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI-SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

*CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)
CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards, five of each suit.

*DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

*P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

•SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

•SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. Common criteria are: a probability value of .01 or less, or a deviation in the expected direction such that the critical ratio is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

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MORALITY, MATERIALISM, AND PSYCHICAL RESEARCH

Our civilization, our moral ideals and standards of conduct, have been built up on the basis and under the guidance of certain definite beliefs that are incompatible with materialism, the belief that our lives have a significance and value that is greater than appears on the surface of things, the belief that we are members of an order of things that somehow is a moral order, and that the value of moral idealism and moral effort cannot be measured in terms of material comfort or the satisfactions of our animal nature. Our moral tradition is the product of such beliefs. There is no good reason to think that, in the absence of such beliefs, any high moral tradition could have been evolved by any branch of the human race. Are we then justified in assuming that, if the foundations are sapped away, the superstructure of moral tradition will continue to stand unshaken and unimpaired, powerful to govern human conduct through the long ages to come?

. . . . . .

If materialism is true, let us ascertain the fact by all means; let the truth be told, though the heavens fall and all the gods also. And let us then hope that civilization may succeed in adjusting itself to this truth and, by its aid, may render human life better worth living. But at present it is clear that the civilized world is becoming more and more acutely divided on this question, the question of the truth of materialism. This lack of sure knowledge, and the consequent wide and widening divergence of opinion is a scandal, a reproach to our boasted scientific culture, and an actual and increasing social danger. Here, then, is one good reason why the convinced scientific materialist should support psychical research.

. . . . . .

Unless psychical research—that is to say inquiry according to the strictest principles of empirical science—can discover facts incompatible with materialism, materialism will continue to spread. No other power can stop it; revealed religion and metaphysical philosophy are equally helpless before the advancing tide. And, if that tide continues to rise and to advance as it is doing now, all the signs point to the view that it will be a destroying tide, that it will sweep away all the hard-won gains of humanity, all the moral traditions built up by the efforts of countless generations for the increase of truth, justice, and charity.

—William McDougall, *Religion and the Sciences of Life*